MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XXXVIII. MARCH 8, 1878. No. 5.

LORD LINDSAY, M.P., President, in the Chair.

John E. Gore, Esq., C.E., M.R.I.A., Asst. Engineer, D.P.W., India, Dromard Rectory, Co. Sligo;

The Rev. Frederic John Hall, Haileybury College, Hertford;

William Hobson, Esq., Park View, Whalley Range, Manchester;

Lieut. Robert Hoggan, R.N., H.M. Dockyard, Sheerness;

William C. Otter, Esq., 60 Grosvenor Park, Camberwell; and

F. H. Stevens, Esq., B.A., Clifton College, Clifton;

were balloted for and duly elected Fellows of the Society.

Letter received by Lord Lindsay from Dr. W. Foerster.

MY LORD,—As the time is approaching when the Annual Report of the Council of the Royal Astronomical Society is published, I beg to call your attention to a passage in the last Report (February 1877, p. 188), where the planetary work of the *Berliner Astron. Jahrbuch* is mentioned in a way which does not do full justice to it.

It is said in this passage that space has only been found in the *Berliner Astron. Jahrbuch* for 50 out of 125 planets which will be in opposition between January 1 and December 31, 1877, "and it is regretted that so few of the older and larger planets are included in the selection."

From these expressions it will be inferred that the *Berliner Jahrbuch* has been liable to certain external and, as it seems by the words "space has not been found," to economical restrictions which have not permitted the superintendents to do the necessary planetary work required by science.

But in every volume of the *Jahrbuch* since the year 1874, and finally in *Circular* No. 44, we have endeavoured to

show by general deductions, accompanied with more and more detailed practical propositions, which have nowhere been seriously refuted and which have been approved by several meetings of astronomers, that the most perfect and exhausting care of the small planets is of prominent scientific interest, and that in the actual state of things this task can only be reasonably fulfilled by the introduction of some restrictions of a scientific nature, but in no way of economical or any other external character.

The intensity of planetary work at Berlin and in the whole astronomical world has not weakened, but has sensibly increased, since these restrictions have been carried into practice. Most astronomers concerned in that department of science have now gained the hope that, by reasonable organisation, and discernment of the essential from the secondary or imaginary wants of science, the whole planetary work in future may be fully mastered, and that thus, by an exhaustive treatment according to our propositions, more and more important results will be furnished for the small planets. It will be seen by the Royal Astronomical Society—especially from the periodic *Circulars* of the *Berliner Jahrbuch*, which now form an essential completion of the annual volume of this publication—that the Superintendency of the *Jahrbuch*, mainly conducted by my friend, Dr. Tietjen, endeavours to do the best possible to aid observers and computers of small planets.

I hope that you will be able to procure for this communication a place in the next publication of the Royal Astronomical Society.

I am, my Lord, with the greatest respect, your obedient servant,

W. Foerster,
Director of the Berlin Observatory.

Berlin,
1878, *February* 10.

Galileo's Trial before the Inquisition in the light of recent Researches.

By Sedley Taylor, M.A., late Fellow of Trinity College, Cambridge.

(*Communicated by J. W. L. Glaisher, Esq.*)

Within the last ten years a large number of previously inaccessible official documents relating to the trial of Galileo before the Inquisition have been made public. The fresh evidence thus far produced has given rise to an entirely new view as to the nature of that proceeding, a short account of which will be found in the following Paper:—

Up to the year 1867, the best informed writers held, almost

to a man, that the Inquisition, whatever might be thought of the wisdom of its proceedings, had a clear legal right to punish the great astronomer for the course which he pursued. The text of the sentence against him stated that he had committed a palpable contempt of court by writing in favour of the Copernican doctrine in 1632 after having in 1616 been formally inhibited by the Inquisition from doing so. As long as this assertion was accepted it was obviously impossible to clear Galileo of the charge of contumacy, by which his condemnation was in the last resort determined. It is a singular circumstance that historians, many of whom were by no means antecedently inclined to repose a childlike faith in the veracity of the Inquisition, did, one and all, accept this its unsupported assertion, and with equal unanimity overlooked a glaring contradiction on the surface of the sentence, which it was reserved for a German writer to point out in 1870.

The view that Galileo was legitimately punished for contempt of court received much support in 1850 from a work published by Marino Marini, Prefect of the Secret Archives of the Holy See, and entitled *Galileo e l' Inquisizione.* It was previously known that a contemporaneous record of the trial, drawn up from day to day by the Notary of the Inquisition, was preserved among its archives: Marini, the official guardian of this record, now for the first time printed a considerable number of extracts from its contents. The object of this writer manifestly was to represent the conduct of the court as conspicuously mild and benignant; that of the accused as intolerably troublesome and rebellious. By a system of really masterly suppression and garbling of the documents to which he alone had access, Marini succeeded in completely misleading historians of the trial for more than a quarter of a century, until, in fact, the publication *in extenso* of the documents manipulated by him exposed his manipulations to the light of day.

The first tolerably complete edition of the trial-record was published in 1867 by *M. de l' Epinois*, to whose latest exertions a few months ago we owe a verbatim issue of the whole. Professor Berti also contributed an edition in 1876, and Herr v. Gebler closes the series with one still more recent, and aiming at even more minute reproduction of the original than its predecessors. The evidence supplied by the official minutes of the trial is therefore henceforth universally accessible except in so far as the genuineness of parts of the manuscript itself may be called in question.

The writer who has made the most important use of these new materials is Dr. Emil Wohlwill, of Hamburg, who published, in 1870, a short work on the trial, and followed it, late in 1877, by a detailed examination of the question whether the prisoner was submitted to the torture during the course of the proceedings. The earlier of these publications investigates an issue of much greater, indeed of cardinal, importance, viz.

whether the Inquisition told the truth when it asserted that Galileo had, in 1616, been formally served with an order of court inhibiting him from defending the Copernican doctrine. Wohlwill maintains that this asserted inhibition had no actual existence, and that the whole incident was a fraudulent fabrication perpetrated in order to secure a condemnation which could not be compassed by legitimate means. That a forgery was committed in one of the documents of the trial-record for the purpose of carrying out this nefarious scheme, and that highly placed officials of the Inquisition were accomplices in the conspiracy, are corollaries which follow immediately from the above position.

Before stating the chief arguments on which the German writer relies, it will be well to premise a concise *résumé* of the essential facts in Galileo's case.

It is probable that about the year 1592 he became a convert to the heliocentric system, which had been propounded half a century before by Copernicus in the *De Revolutionibus Orbium Cœlestium*. He seems, however, for a considerable time after this change of view, to have continued publicly to teach the Ptolemaic system, and this, as he himself explained in a letter to Kepler, "on account of the huge number of fools" and the derision and molestation which adherents of the new doctrine would be sure to experience at their hands. But by slow degrees he must have allowed his genuine convictions to appear, for in 1615 we find him furiously attacked from the pulpit for his adherence to the new astronomy. The preacher, a monk at Florence, denounced mathematics as a diabolical art, and sought to enlist the force of ridicule on his side of the question by quoting the verse at the opening of the Acts of the Apostles—

> Ye men of Galilee, why stand ye gazing up into heaven?

as a punning hit against the star-gazing disciples of Galileo. It ought, perhaps, in justice to this monastic punster, to be remarked that his joke sounds decidedly better in the Latin of the Vulgate than it does in the English of the Authorised Version. "Galilei," the Latin equivalent for "Galileans," is identical with the proper surname of the philosopher, whose complete designation was Galileo dei Galilei. By some odd freak of custom we in England nvariably call him by his Christian name only.

The enemies of our astronomer unfortunately did not confine their mode of warfare to sermons and Biblical puns—an artillery whose fire he no doubt was prepared to sustain with considerable equanimity. The monks of the monastery of St. Mark concocted a letter to the Inquisition at Rome, in which they denounced to that formidable tribunal some opinions which Galileo really did hold, and a great many more which he did not. The Holy Office, once put on the scent of heresy, set to work with its usual keenness, and before long Galileo received a private hint from friends in Rome that some underground machination was going on

against the new astronomy. On this intimation he went up to the capital, in the hope that, by pointing out to the authorities the futility of attempting to arrest the progress of the new views by ecclesiastical censures, he might save the rulers of the Church from the humiliation of publishing decrees against the laws of nature. Galileo overestimated his powers of persuasion. The absurdity which he strove to avert was resolved upon, and the formal decree, declaring the Copernican doctrine to be false and contrary to Scripture, was issued by the Congregation of the Index on the 5th of March 1616.

It is very generally supposed that by this decree the work of Copernicus was prohibited, but this is a misconception. The *De Revolutionibus* was only ordered to be "suspended until corrected," i.e. withdrawn from circulation for the purpose of having certain alterations made in its text under the direction of the Congregation of the Index. The list of corrections subject to which the work was permitted to reappear was published in 1620; it is very short, covering, perhaps, two pages out of two hundred. The object aimed at by the correctors was manifestly the introduction of *hypothetical* language where the author had spoken *categorically*. Much skill is shown in giving this turn to a number of passages by the slightest verbal alterations, and it is only a very few paragraphs which, as not susceptible of such treatment, were ordered to be expunged. The fact that the treatise of Copernicus was allowed to reappear with only these superficial readjustments, which left its long chain of reasoning untouched in a single essential link, shows conclusively that the Congregation, though determined to get rid at a single stroke of the vexatious discussions likely to arise about the relations between Scripture and the new astronomy, did not demand more than a certain decorum of overt expression from writers who discussed the question from a purely scientific point of view.

In 1632 Galileo published his Dialogues on the Ptolemaic and Copernican systems, which led directly to all the misfortunes and sufferings of his remaining years. The work was suspended about seven months after its appearance by a peremptory order from Rome, and two months later its author received a citation to appear before the Supreme Tribunal of the Inquisition. The result of the subsequent trial is well known. The accused was declared to have rendered himself vehemently suspected of heresy, compelled to abjure, and condemned to incarceration during the pleasure of the Court: a punishment which was, however, promptly commuted to practical imprisonment in his own remotely situated villa at Arcetri, where he dragged out his remaining years under the humiliating *surveillance* and repeated menaces of the Holy Office. The main charges on which the prisoner was convicted were the following:—

(1) That the Dialogues transgressed the limits laid down by the decree of the Index.

(2) That they were in violation of a private personal in-

junction, containing restrictions far more stringent than those of the public decree, which the tribunal asserted to have been formally delivered to Galileo on February 26, 1616, by the then acting Commissary-General of the Holy Office.

To the first allegation the prisoner had a complete answer. He had submitted his work in manuscript to the Papal licensing authority, and obtained the regular *Imprimatur* before proceeding to publication, and therefore could not be made criminally responsible for its contents, inasmuch as it was manifestly not his business, but that of the licenser, to know what was and what was not compatible with the decree of the Index.

The second article of charge depends, of course, on the exact terms of the notice given to Galileo on February 26, 1616. The nature of the whole asserted proceeding appears from the following entries, translated from the Latin of the trial-record :—*

"Thursday, Feb. 25, 1616.—The Most Illustrious Lord Cardinal Millinus gave notice to the Rev. fathers, the Lords Assessor, and Commissary of the Holy Office that, on the censure of the Fathers Theologians passed upon the propositions of Galileo—that the Sun is the centre of the universe and locally motionless, and that the Earth moves even with a daily motion—being reported, the Most Holy ordered that the Most Illustrious Lord Cardinal Bellarmine should summon to his presence the said Galileo, and admonish him to abandon the said opinion, and that, if he should refuse to obey, the Father Commissary, in the presence of a Notary and Witnesses, should deliver to him a precept entirely to abstain from teaching or defending such doctrine or opinion, or from treating thereof; but that if he did not acquiesce he was to be imprisoned.

"Friday, Feb. 26 of same year.—In the Palace habitually occupied by the said Most Illustrious Lord Cardinal Bellarmine, and in the apartments of his Most Illustrious Lordship the above-named Galileo having been summoned, and himself being in the presence of his Most Illustrious Lordship, and in the presence of the very Rev. father Brother Michael Angelo Seghitius of Lodi, of the Dominican order, Commissary-general of the holy office, the Same Most Illustrious Lord Cardinal admonished the above-named Galileo concerning the error of the above-named opinion, and to abandon it; and thereupon and forthwith, in the presence of myself and Witnesses, and the same Most Illustrious Lord Cardinal being likewise still present, the *Father Commissary enjoined upon and ordered the aforesaid Galileo, he being still in the same place and under formal Presentment, in the name of Our Most Holy Lord Pope and of the whole Congregation of the holy office, entirely to abandon the above-named opinion that the Sun is the centre of the universe and motion-*

* The paragraphs thrown into italics are those which Wohlwill considers to be a subsequent and fraudulent addition.

less and that the Earth moves, and in Future not to hold, teach, or defend it in any manner whatever, by word of mouth or in writing, failing which proceedings would be taken against him in the Holy office; to which precept the same Galileo submitted and promised obedience.—Done at Rome as above, there being present at the same place R. D. Badinus Nores of Nicosia in the Kingdom of Cyprus, and Augustinus Mongardus, of a place in the Abbacy of Rose, in the diocese of Montepulciano, servants of the said Most Illustrious Lord Cardinal, as Witnesses."*

The above minutes are quoted verbatim in the sentence of 1633, as first-hand evidence of what occurred in 1616, and the whole incident of the Commissary's inhibition rests on no other authority. Everything therefore turns on the question whether this incident was a fact or a fiction. Wohlwill's reasons for adopting the latter of these alternatives shall next be summarily stated.

An unsuspecting reader might easily take the minute of February 26 as describing a straightforward carrying out of the order laid down in that of February 25. It can, however, be so regarded only by making an arbitrary assumption which will presently be shown to be contradicted by positive evidence. The Papal order contained in the first minute provides that the Commissary was only to intervene officially on the occurrence of a particular contingency, viz. on Galileo's refusing to obey Bellarmine's admonition. The second minute represents the Commissary as delivering his "precept" immediately upon the conclusion of the Cardinal's address, and, as the language used seems to imply, without even waiting to ascertain whether Galileo refused to obey or not. It is impossible to suppose that the Commissary would have dared, in the presence of the leading Cardinal of the Holy Office, thus to set at nought the direct instructions of the Pope himself. If therefore the two minutes are to be reconciled at all, this can only be done by assuming, without a tittle of evidence, that Galileo, when admonished by the Cardinal, refused to submit, and so brought down upon himself the Commissary's precept.

But this assumption is put out of court by a piece of direct evidence which came to light under singularly interesting circumstances. In 1848, when the suspension of the temporal power at Rome placed the secret archives of the Vatican for a brief interval within the grasp of the revolutionary government, Professor Silvio Gherardi, one of the Secretaries of State, seized the opportunity to institute a search among the records of the Inquisition with a special eye to documents bearing on the case of Galileo. After an extremely laborious exploration he succeeded in unearthing a considerable number, which, as subsequent comparison has shown, were not included in the collection since published, and probably formed part of an entirely distinct series.

* The text of *v. Gebler's* edition has been followed.

One of these documents relates how, on March 3, 1616, Cardinal Bellarmine reported as follows to the Pope:—"Galileo Galilei, mathematician, admonished by order of the Sacred Congregation to abandon the opinion which he has hitherto held, that the Sun is the centre of the spheres and immovable, but the Earth movable, acquiesced."

No words are needed to enforce the decisive bearing of this pregnant sentence. It is evident that Bellarmine is here reporting his execution of the order contained in the minute of February 25, and its successful result. Galileo had acquiesced under his admonition, and the contingency which was to entail the Commissary's intervention had not arisen. The attempt to reconcile the conflicting minutes by assuming that Galileo refused to submit must therefore be abandoned. It will be noticed how Bellarmine's report corroborates the minute of February 25 and the earlier part of that of February 26, while it invalidates the latter's disputed close.

Singularly enough, the great Cardinal has left behind him a further piece of testimony equally destructive of the *rôle* attributed to the Commissary. Having been applied to by Galileo, a few months after the interview, for a memorandum stating exactly what had occurred, Bellarmine forwarded an autograph certificate that only the declaration published by the Index Congregation (i.e. their decree of March 5 on the Copernican theory) had been announced to Galileo. It is manifest that this statement entirely excludes the delivery to him of a further and infinitely more stringent prohibition, such as the Commissary's precept. The Court in their sentence quoted Bellarmine's certificate as genuine and authentic, and by so doing implicitly invalidated the asserted action of the Commissary, on which, nevertheless, they persisted in condemning the prisoner. It is this flagrant contradiction between two pieces of evidence, each insisted on by the Inquisition as trustworthy, to which reference has already been made as having been for the first time pointed out by Wohlwill.

The evidence given by Galileo during his examinations by the officials of the Holy Office, and the statements of his written defence handed in to the tribunal, minutely confirm the position that no such prohibitory order as the precept of 1616 had ever reached him. In the interrogatory of April 12, 1633, the efforts of the examining official were specially directed towards eliciting an admission of the delivery on February 26, 1616, of the Commissary's precept. The examiner manifestly held in his hand the minute the genuineness of which is now called in question, and sought, by reading aloud portions of its text, to recall the incident it described to the memory of the accused. His attempts were fruitless. Galileo, though plainly anxious to show the utmost docility to the wishes of the Court, and willing to accept the technical term "precept" ("*precetto*"), by which the examiner designated the communication made to him, asserted that he had no recollection of the precept in question having

been delivered to him by any other person than Cardinal Bellarmine.

In his defence, presented to the Court on May 10, Galileo remarks that the authentic attestation of what took place on February 26, 1616, which he possessed in Bellarmine's certificate, did not contain a trace of an indication that any injunction had been laid upon him save that general pronouncement which concerned everyone alike—viz., of course, the decree of the Index.

These statements appear to be supported by the strongest internal probabilities. We may at once set aside the notion that Galileo may have *forgotten* the Commissary and his message. His detailed statements on the multifarious subjects as to which he was examined show that he possessed a memory of unusual tenacity. When we think what in Italy, in the seventeenth century, it was in the power of the Inquisition to inflict on those who incurred its displeasure, we cannot doubt that, had Galileo been actually brought face to face with a leading official of the dreaded tribunal, and received from him the order of the Court, ending with the warning "Obey, or a suit will commence against thee in the Holy Office," the scene must have been branded into his memory in letters never to be effaced while life and memory endured.

It may indeed be urged, and has been urged, that Galileo, though conscious that he had received the Commissary's precept, chose deliberately to deny it. Now as, unfortunately, it cannot be denied that, at a later point of his examination, Galileo was betrayed into asserting on oath what was unquestionably false, there is nothing antecedently improbable in the suggestion that he may be doing the same thing here. Nevertheless there are, I think, indications which point decisively to an opposite conclusion.

We may assume that a sensible man, to say nothing of one of eminent tact and acuteness, will not, however unscrupulous he may be, tell a deliberate lie by which little or nothing is to be gained, especially if by doing so he is likely to expose himself to serious and imminent peril. Now Galileo, before advancing the statements which we are discussing, was made aware by the official who examined him that the Court was in possession of a formally attested record of what took place on February 26, 1616. He must have felt, therefore, that it was useless to deny what thus admitted of direct legal proof—and not merely useless, but excessively dangerous; it being the practice of the Inquisition to resort to the application of the torture when there was reason to suspect that a witness was not telling the truth. For these reasons, if Galileo had been conscious of having really received the Commissary's precept, he would surely not, in the face of the official minute which he saw in his interrogator's hand, have ventured on the desperate course of denying all knowledge of it.

There is a further argument to which considerable weight ought

to attach. A person who reads through the whole of Galileo's answers on April 12 will hardly fail to reach the conclusion that he did not really know what his examiner meant by the "precept," and fancied he alluded to a particular form of words employed by Cardinal Bellarmine, and that the question was whether or not certain crucial expressions, such as "to *teach*" and "in *any* manner," were or were not included. On the whole, the balance of probability inclines very decidedly in favour of the truthfulness of Galileo's asseverations. They contain, therefore, strong evidence for the non-delivery of the precept of 1616.

In addition to the direct evidence which has been already detailed, Wohlwill has collected a considerable body of collateral indications which tend to exclude the asserted action of the Commissary. Some of these shall next be briefly stated.

The fact that Galileo was a devout and warmly attached member of the Roman Church affords some antecedent presumption that he would not have refused to yield to an admonition from her greatest Cardinal, with whom too he stood on a footing of private friendship. But further, in his private letters he is found repeatedly congratulating himself on the docile attitude he had assumed towards the Church on the Copernican question. Thus, in a letter written on March 6, 1616, he says that his conduct throughout the whole transaction had been such that "no saint could have borne himself with greater reverence or greater zeal towards Holy Church." This is hardly the language of a man who but ten days before had met Bellarmine's admonition with a refusal to acquiesce.

The same asseverations reappear in a letter written some two years after his condemnation, when all hopes of softening the hearts of his persecutors by professions of zeal for the Church must, if he ever entertained them, have utterly vanished :—

"I have said," he writes, "that I do not hope for any alleviation, and this because I have committed no crime whatever: I might have hoped to obtain grace and pardon had I erred; for transgressions afford to princes the means of exercising mercy and indulgence; but in the case of a man unjustly condemned, the rigour of the sentence must be sustained in order to keep up an appearance of its having been legitimately arrived at. This afflicts me less than others may think, because two consolations constantly support me. One of these is that there is not to be found in all my writings the least shadow of lack of piety or of reverence for Holy Church. The other is my own conscience, fully known only to myself on earth and to God in heaven, who well knows that in the cause for which I suffer many might have acted and spoken with greater learning, but not one, even of the holy fathers, more piously nor with greater zeal towards Holy Church, nor, in short, with a more holy intention, than I."

From a letter of Galileo, dated March 12, 1616, we learn that on the previous day he had a most gracious audience of the

Pope, who, in reply to some expressions of alarm let fall by Galileo at the prospect of being the object of malignant and implacable attacks, assured him that he was so well thought of by himself and by the whole Congregation that credit would not be lightly given to the persecutors.

If on February 26 Galileo had met the admonition of Bellarmine with a refusal to submit, and had thereupon been visited by the Commissary, "in the name of our lord Pope and of the whole Congregation of the Holy Office," with a peremptory and threatening inhibition, it is all but inconceivable that the Pope should, within a fortnight, have assured him how well he stood, "both with himself and the whole Congregation."

Galileo's subsequent conduct was moulded most carefully in conformity to the Index decree, but was quite inconsistent with the terms of the order put in the mouth of the Commissary. Thus, when in May 1618 he presented to the Archduke Leopold of Austria a copy of his treatise on the tides, he was careful to accompany it with a statement that, since it was becoming to obey and believe in the decisions of the authorities, he wished any argument of his which rested on a Copernican basis to be regarded as a piece of poetry or a dream. It is manifest that this, though a literal compliance with the demands enforced against Copernicus by the Index Congregation, was no compliance at all with a precept not to teach or defend heliocentricism "in *any manner*, in writing or by word of mouth."

In 1624 Galileo wrote an elaborate answer to a letter against the Copernican system which had been addressed to him in 1616 by a certain *Franceso Ingoli*, who had in the meantime become secretary to the Congregation of the Propaganda. Here again he explicitly disclaims any intention of maintaining and holding as true that proposition which had been declared inconsistent with doctrines more majestic and authoritative than those of the natural sciences, and says that his object in writing was to show that, when he was engaged with astronomers and philosophers on this question, he was neither so blind of intellect nor so feeble in argument as to have taken the Copernican side for lack of having perceived, or understood, the instances produced by Ingoli. He then administers a severe rebuke to the unlucky secretary, telling him that, for every day he had devoted to the subject in hand, Copernicus had devoted a year, and that he must not lightly imagine himself able to overthrow so great a man as Copernicus, especially with such weapons as those he employs against him, which are objections of the commonest and most trivial description. After this he sets to work deliberately to tear the poor man's arguments to pieces one by one, for some forty pages or more. A copy of this production was forwarded by Galileo to Ciampoli, secretary to Pope Urban VIII. (who had succeeded Paul V. in 1624). It is difficult to suppose that Galileo, with a threat of proceedings in the Holy Office hanging over his head, would have dared to send to the Pope's secretary

such a barefaced violation of the precept delivered him by the Commissary.

Still more decisive is the history, too long to be gone through in detail, of the negotiations which preceded the publication of the great "Dialogues." Galileo unhesitatingly placed in the hands of the licensing authority at Rome a work, the very title of which claimed the right of discussing the Doctrine of Copernicus, subject only to the limitations enacted by the Index Congregation, to whose decree the preface contains a formal adhesion. The work, after being subjected to minute and reiterated inspection, received the final approval of the licensers, and appeared with their Imprimaturs conspicuously displayed on its first page.

The conduct of the licensers is perfectly intelligible on the assumption that the decree of the Index alone guided their scrutiny, but utterly unaccountable on the opposite supposition. The only possible object which the Inquisition could have had in laying Galileo under the special personal restrictions detailed in the Commissary's precept was to bind him over to absolute silence about the condemned doctrine. It seems highly improbable that, in a case of such importance, an order like that of the Commissary should never have been notified to the Pope's licensers of books, should have become a dead letter, and within sixteen years been so utterly forgotten as to have been only "discovered" in a search among the archives of the Inquisition, instituted after the appearance of Galileo's "Dialogues."

The general character of the collateral evidence will be clear from the above summary. There remains only one more point of great importance for consideration, viz. the indications of genuineness or spuriousness afforded by the state of the manuscript trial-record itself as far as regards the disputed document. In his new edition, M. de l'Epinois has given a photo-lithographic facsimile of the minutes of February 25 and 26, and thus enabled other persons besides the exceedingly select few who have as yet been permitted to inspect the originals to form some idea of the evidence presented by their present condition. In the opinion of Wohlwill the result of a careful examination strikingly confirms his view. The first part of the minute of February 26, down to the Latin word translated "whole" in the version given above (pp. 260, 261), occupies the lower half of a left-hand page (fol. 378) of the manuscript; the remainder of it the adjoining right-hand page (fol. 379). The last two lines of fol. 378, in which the intervention of the Commissary is narrated, present, when examined under a magnifying glass, certain very peculiar phenomena which occur in no other part of the facsimile. A number of dots, and what look like remnants of older writing, are seen peeping out between the letters, and parts of letters, of the present text, and there is a blurred appearance about certain of the words, and notably about the all-important word "Commissary," which looks very much as if it were the result of

writing over an erasure. Wohlwill's theory, which he supports by an investigation too technical for reproduction here, is that the minute of February 26 originally terminated at the foot of fol. 378 with a statement that Galileo had acquiesced under Bellarmine's admonition. The last two lines which contained it were then, he thinks, operated on by partial erasure and re-writing, including the use where possible of some of the old words or letters, so as to bring them into their present form. The fraud was then easily completed by writing the rest of the spurious minute on the next page, which, like many others in the manuscript, may very probably have been originally left blank. In doing this the genuine handwriting was carefully imitated, but, as Wohlwill confidently maintains, with but incomplete success.

It is proper to remark here that two of the three editors of the manuscript, Berti and v. Gebler, assert the genuineness of the disputed minute, while the third, De l'Epinois, has as yet expressed no opinion on the point. This fact affords some presumption against the view of Wohlwill, which was arrived at without personal access to the impugned document. A thorough re-examination of its contents by skilled and trustworthy palæographers alone offers a prospect of settling the question.

It is obvious that this new view of Galileo's trial involves the gravest charge against the Inquisition, that, viz., of having manufactured a spurious basis for a criminal indictment, and consummated a cruel wrong on the ground of an iniquitous fraud. If this charge is once established, it will be no easy matter to exonerate even the Cardinals of the supreme Congregation itself from complicity in the crime.

Clearly, therefore, the authorities of the Vatican will do well, unless they desire judgment to go by default, to produce additional documentary evidence, and bring this plain issue, so deeply affecting the good faith of the Holy Office in by far the most famous case ever decided at its bar, to a definitive settlement. The object of the few remaining lines devoted to this subject here will be to show that important unpublished evidence in the case unquestionably once existed, and presumably still exists, among the archives of the Inquisition.

It has been made abundantly clear by the most recent researches that the published trial-record is far from complete, that some of its documents are only copies or abstracts, and that others show plain marks of a later origin than the rest of the collection. The sentence and form of abjuration are both missing, as are also the opinions given by the consulting jurists of the Inquisition, though it is known from the Court's own statement that these opinions were actually obtained. Further, there is no report of the sittings held on the case by the supreme Congregation. The short references to decisions there arrived at are mere unauthenticated summaries. The original formally attested records of all these must once have formed part of the Inquisitional archives, and ought to be forthcoming now.

On Hansen's Terms of Long Period in the Lunar Theory.

By E. Neison, Esq.

Recent investigations have shown that the theory of the terms of long period in the motion of the Moon is at present in a most unsatisfactory condition, so that the entire subject urgently requires further and more extended investigation. When Hansen announced that he had discovered the existence of two terms of long period in the motion of the Moon, due to the perturbations of *Venus*, it was thought that the last difficulty in the lunar theory had been overcome. Hansen's tables were found to completely represent the motion of the Moon for the period 1750–1850, and this was regarded as the most satisfactory evidence of the correctness of the theory on which they were based.

Unfortunately, in a most important point Hansen's tables do not really rest on his theoretical investigations. Purely empirical values were assigned by Hansen to the coefficients of the two important terms of long period which he had discovered to be produced by the planet *Venus*. Referring to these terms, Hansen says: "La détermination exacte de ces deux inégalités par la théorie est la chose la plus difficile que l'on rencontre dans la théorie du mouvement de la lune. J'ai cherché deux fois à déterminer leurs valeurs, et par des méthodes différentes, mais j'ai obtenu des résultats essentiellement différents l'un de l'autre." (*Monthly Notices* vol. xv., Nov. 1854.) For this reason, therefore, Hansen thought it best to give such empirical values to these terms as was necessary to bring theory and observation into accord during the interval 1750–1850. In this respect, therefore, Hansen's tables are decidedly unsatisfactory, and the fatal consequences of this course have swiftly overtaken his tables. The values for the coefficients of these two terms of long period, which Hansen obtained from his theoretical investigations, appear to have differed considerably from the empirical values employed by him in his tables. Hansen has stated that he intended undertaking a third investigation for the purpose of satisfactorily determining the theoretical value of these two coefficients. The results of this new investigation have never been made known, and it is doubtful if it was ever completed.

In 1858 Delaunay was led to take up the examination of these two terms of long period in the motion of the Moon, which had been discovered by Hansen to be produced by the action of *Venus*, for he remarks the lunar theory could not be left in the unsatisfactory condition mentioned by Hansen. Delaunay arrived at most important results; for whilst he confirmed from theory the value which Hansen had empirically assigned to the coefficient of the term of long period with the argument $(g - 18g'' + 16g')$, using Hansen's notation (*Conn. des Temps*, 1862, App.), he found that the theoretical value of the coefficient of the other term, whose

argument is $(8g''-13g')$, was so small that it might be regarded as insensible. (*Conn. des Temps*, 1863, App.)

This was a very serious matter; for if Delaunay's results were correct, they showed that there must be grave errors in the theoretical basis of Hansen's *Tables de la lune*. And if this were so, these tables could not continue for long to represent the place of the Moon. In 1870 Professor Newcomb pointed out that Hansen's tables failed apparently to represent the observations anterior to 1750, and he has lately confirmed this by a more extended investigation, in which he clearly shows that great discordances exist between the observed position of the Moon and the position assigned by Hansen's tables. What is of still more importance is the fact that of late the places of the Moon assigned by Hansen's tables have deviated very considerably from the observed place, and this deviation is rapidly increasing. It is evident, therefore, that although by giving empirical values to the coefficients of the two terms of long period which he had discovered to be produced by the perturbations of *Venus*, Hansen had been enabled to make his tables represent the observed place of the Moon during the interval 1750–1850; yet this had only been effected by means which had rendered his tables discordant prior and subsequent to this period.

It is certain, therefore, that there is some grave deficiency in Hansen's *Tables de la lune*. This fact strongly confirms the accuracy of Delaunay's conclusions with respect to the value of the terms of long period, but only to this extent, that Hansen's empirical values are wrong. For if Delaunay's values of these terms be adopted, and Hansen's tables be corrected, they still fail to represent the motion of the Moon, with this important difference, that they do not represent it for any period whatever. Either, therefore, both Hansen's and Delaunay's values are wrong, or there still remains to be discovered one or more important terms of long period in the theory of the Moon.

The determination of the value of the coefficients of the terms of long period in the motion of the Moon is an exceedingly laborious and difficult work. Not only are the analytical developments which are involved much more complex than in any other branch of the lunar theory, but they are of very great length. Generally the value of a term of long period is the difference between two large quantities of very nearly equal magnitude. These quantities are usually twenty to thirty times greater than the coefficient to be determined, and are in turn composed of a large number of terms of considerable magnitude and partially destroying each other. Consequently, the neglect of some comparatively insignificant term, or the slightest error of any kind, may be sufficient on one hand to quadruple the true value, or on the other hand to reduce it to zero. The numerical coefficients which are involved in these terms must be carried to a point of extreme accuracy, for I have found the value of an important term to be vitiated from my having carried these numerical coefficients only

to the fifth decimal place, instead of to the seventh decimal place, although accuracy to the third decimal place appeared to be ample. It is true that these difficulties are present in a very mitigated form in the two terms of long period which were discovered by Hansen. Yet, as the remarks quoted from Hansen show, their determination was found by him to be the most difficult work he had encountered in the lunar theory. Moreover, Poisson and Pontécoulant were unsuccessful in determining the value of one of these two terms, and Delaunay himself failed in his first attempt to calculate the value of the other. All three failures were due to their having omitted some apparently insignificant factor, and so obtaining a value far less than the truth. It was probably on account of the known difficulty of the problem that Delaunay's investigations do not seem to have attracted the attention that was merited by the importance of their subject.

It is now generally admitted that the value which Hansen assigned to the coefficient of the term of long period whose argument he denoted by $(8g''-13g')$ is far greater than its true value, and that we must abandon this second term of long period which is produced by the planet *Venus*. Delaunay's conclusion that its coefficient was very small has been completely confirmed by the entirely independent researches of Professor Newcomb. My own results are in accordance with those of Delaunay, and I have sufficiently verified the analytical developments employed by Delaunay in his memoir to satisfy myself that they are not vitiated by one of those accidental errors from which even the most careful work of the finest mathematicians is not necessarily exempt. It may be regarded as certain, therefore, that Delaunay's investigations are free from any serious error. Yet I understand that Hansen would never admit that Delaunay's researches established the inaccuracy of his own earlier investigations, although I cannot learn that Hansen ever definitely stated the grounds of his objection. It has always appeared to me that Hansen must have had some good reason for his objection, but that he was waiting until he had completed his third investigation of this difficult problem.

My own investigations, whilst they completely confirm the accuracy of Delaunay's result, have led me to the unexpected conclusion that Hansen may be correct after all. Are Delaunay's investigations complete?

I do not refer to the terms depending on the square of the mass of *Venus*, and to which Professor Newcomb has drawn attention in his remarkable paper in the *American Journal of Science* for November 1877, because all investigators have hitherto neglected them, so that it cannot be these terms which produce the discrepancy between Delaunay and Hansen. But I doubt if Delaunay's investigations are really complete as far as terms depending on the first power of the mass of *Venus*, and it appears to me that there is strong evidence that it was from these very

terms, omitted by Delaunay, that Hansen obtained the large coefficient of the second *Venus* term—that with the argument $(8g''-13g')$.

In the *Memoirs of the Royal Astronomical Society*, vol. xvi., Hansen has published the only existing details of his method of computing these inequalities, and on page 476 there occurs the following passage. Speaking of the term whose argument is denoted by him by $(g-18g''+16g')$, and whose value as far as the first power of the disturbing forces he had just computed, Hansen says: "Cependant le cube des forces perturbatrices, ou plutôt le produit du carré et du cube de la force perturbatrice du soleil par la force perturbatrice de *Venus*, ajoute une quantité sensible à ce coefficient, et rend en même temps sensible le coefficient de l'argument $(8g''-13g')$, qui, en tenant seulement compte de la première puissance de la force perturbatrice, serait insensible dans le mouvement de la lune." Delaunay appears to me to have entirely overlooked the real bearing of this important statement, for I think it clearly defines from whence Hansen obtained the large value for the coefficient of the argument $(8g''-13g')$, which was found to be insensible by Delaunay.

The direct action of the planet *Venus* in producing perturbations in the motion of the Moon may be analytically represented by a particular disturbing function made to form part of the complete disturbing function which represents the action of every disturbing body on the Moon. This particular disturbing function depends on the undisturbed geocentric coordinates of the Moon and *Venus* and on the complete perturbations of the Moon. By neglecting the second and higher powers of the mass of *Venus*, this particular disturbing function is developed in terms of the heliocentric elliptic coordinates of *Venus* and the Earth, the geocentric elliptic coordinates of the Moon, and of the different powers of the perturbations in the motion of the Moon which are produced by the disturbing action of the Sun. Delaunay has exclusively confined himself to this portion of the complete disturbing force when finding the direct action of *Venus* upon the Moon, and, as far as the first power of the disturbing forces, it is evident that this is the only portion which need be considered. As far as the first power of the disturbing forces, Delaunay finds a value for the term $(g-18g''+16g')$ which is sensibly identical with that found by Hansen under like conditions. For the term $(8g''-13g')$ under these conditions Delaunay finds its coefficient is insensible. In this also he agrees with Hansen, as the paragraph which has been quoted will show. To this order Delaunay's investigations must be complete, and to this order both Hansen and Delaunay have obtained identical results.

When Delaunay takes into consideration the effect of the different powers of the perturbations in the motion of the Moon arising from the disturbing action of the Sun, in all cases he considers the effect of the first power of these perturbations, and in one particular term he includes the second power of these pertur-

bations. Delaunay shows that these terms do not materially augment the value of the two terms of long period. Granting that these results are perfectly accurate, and there can be no real doubt about their accuracy, is there any discrepancy between them and those which Hansen obtained? It cannot be said that there is; for in the paragraph which has been quoted, Hansen explicitly states that the coefficient of the term $(8g''-13g')$ is insensible unless there be included the terms depending on the product of the first power of the disturbing force of *Venus* with the square and cube of the disturbing force of the Sun. But Delaunay has entirely omitted the terms of this order in his investigations. Delaunay himself does not seem to have noticed this discrepancy, partly because he seems to call the first power of the disturbing force of *Venus* also the first power of the disturbing force of the Sun. And his investigations being performed with the aid of his new and elegant method of attacking the lunar theory, in this portion they are not easily followed by anyone not perfectly familiar with this most laborious method. Had this not been so, it is probable that attention would have been directed before now to this discrepancy.

There is little difficulty in ascertaining what were probably Delaunay's reasons for omitting these terms. It is obvious that the terms neglected will be smaller than those which he has retained, and which depend on the first power of the disturbing force of the Sun. But these last are very small, so that there could be no valid reason for including the others which were still smaller. Delaunay was therefore perfectly justified in omitting these terms. But this very circumstance is enough to suggest the inquiry whether Hansen was really referring to these terms, or whether, in his remarks, he does not refer to some entirely distinct class of terms whose existence has been overlooked by Delaunay. Is there, then, a second class of terms to which Hansen's remarks may really apply? If there is, it at once affords a clue to an explanation of the discrepancy between the results obtained by Hansen and Delaunay, for the latter has confined himself entirely to the one particular class of terms which has been described above.

It appears that there is another and still more direct way in which the disturbing action of the Sun will influence the perturbations in the motion of the Moon produced by the disturbing action of a planet.

Let ρ = geocentric distance of the disturbing body; H = geocentric angular distance between the disturbing body and the Moon, r, v = geocentric radius vector and longitude of the Moon; then the disturbing function representing the action of this body upon the Moon will be

$$R_1 = M_1 \left\{ \frac{r^2}{\rho^3} \left(\frac{3}{2} \cos^2 H - \frac{1}{2} \right) + \cdots \right\},$$

where M denotes the mass of the disturbing body. Suppose the

inclination of the orbit of the Moon and that of the disturbing body to be so small that the product or square of the inclinations may be neglected. Let r_1, v_1 denote the heliocentric radius vector and longitude of the disturbing body, and r_o, v_o the heliocentric radius vector and longitude of the Earth. Then

$$\rho = \{r_1^2 + r_o^2 - 2r_1 r \cos(v_o - v_1)\}^{\frac{1}{2}},$$

$$\cos H = \left\{\frac{r_1}{\rho}\cos(v - v_1) - \frac{r_o'}{\rho}\cos(v - v_o)\right\}.$$

Substituting this value of cos H in the expression for R_1, it becomes

$$R_1 = M_1\left\{\frac{1}{4}\cdot\frac{r^2}{\rho^3} + \frac{3}{4}\frac{r^2}{\rho^5}[r_1^2\cos(2v - 2v_1) + r_o^2\cos(2v - 2v_o) - 2r_1 r_o\cos(2v - v_1 - v_o)]\right\}.$$

This will be the form of the disturbing function which expresses the direct action of a planet upon the Moon, and it is to this portion of the complete disturbing function that Delaunay has entirely confined his attention. The disturbing function which expresses the disturbing action of the Sun upon the Moon will be found by making $r_1 = 0$ in the above. It will be

$$R_o = M_o\left\{\frac{1}{4}\cdot\frac{r^2}{r_o^3} + \frac{3}{4}\frac{r^2}{r_o^3}\cos(2v - 2v_o)\right\}.$$

Let the complete disturbing function be denoted by R, and suppose it to be developed in a series of cosines in the usual manner,

$$R = R_o + R_1 = \Sigma R_o \cos\beta_o + \Sigma R_1 \cos\beta_1 = \Sigma R\cos\beta.$$

Throughout the subscript $_o$ will be employed to denote quantities arising from the disturbing action of the Sun, and the subscript $_1$ to denote all quantities arising directly or indirectly from the disturbing action of the planet. Let R^o denote the value of R when the perturbations of the coordinates are neglected; it is thus necessary to develop R in terms of R^o and the perturbations. Both R_o and R_1 involve the lunar coordinates in identically the same manner, so that their differentials with respect to these coordinates will possess the same identity. Direct differentiation gives

$$\left(\frac{1}{r}\right)^n \frac{d^n R_1^o}{d\left(\frac{1}{r}\right)^n} = (-1)^n.\Gamma(n+2).\Sigma R^o\cos\beta,$$

$$\frac{d^n R}{dv^n} = 2^n.\cos n\pi.\Sigma R^o\cos\beta - 2^n.\sin n\pi.\Sigma R^o\sin\beta.$$

The differential with regard to v vanishes for those terms in the disturbing function not arising from the part of the disturbing function involving v, that is for all terms whose arguments are independent of the angular distance of the Moon and the Sun or other disturbing body. For the present purpose there is no need to separate these two portions, as the distinction can be remembered.

Suppose the complete perturbations of the Moon to be expressed by

$$r\delta\frac{1}{r} = \Sigma a_0 \cos \alpha_0 + \Sigma a_1 \cos \alpha_1, \qquad \delta v = \Sigma b_0 \sin \alpha_0 + \Sigma b_1 \sin \alpha_1.$$

To develop R let

$$D^0{}_r = \left(r\delta_0 \frac{1}{r}\right)\left(\frac{1}{r}\cdot\frac{d}{d\frac{1}{r}}\right), \qquad D^1{}_r = \left(r\delta_1 \frac{1}{r}\right)\left(\frac{1}{r}\cdot\frac{d}{d\frac{1}{r}}\right),$$

$$D^0{}_v = (\delta_0 v)\left(\frac{d}{dv}\right), \qquad D^1{}_v = (\delta_1 v)\left(\frac{d}{dv}\right),$$

then a known symbolical expansion gives

$$\begin{aligned} R &= R^0 + \delta R \\ &= \left\{^{D^0{}_r + D^1{}_r + D^0{}_v + D^1{}_v}\right\} R^0 \\ &= \left\{\epsilon^{D^1{}_r}\cdot\epsilon^{D^1{}_v}\right\}\left\{\epsilon^{D^0{}_r}\cdot\epsilon^{D^0{}_v}\right\} R^0 \\ &= \left\{(1 + D^1{}_r + \cdots)(1 + D^1{}_v + \cdots)\right\} \times \\ &\qquad \left\{(1 + D^0{}_r + \frac{1}{\lfloor 2}\cdot(D^0{}_r)^2 + \cdots)(1 + D^0{}_v + \frac{1}{\lfloor 2}(D^1{}_v)^2 + \cdots)\right\} R^0. \end{aligned}$$

Replace D and its powers and products by their values and perform the operations, then the expression for R becomes

$$R = \Sigma R^{o}\{[\cos\beta - 2(a_o\cos\alpha_o)\cos\beta - 2(b_o\sin\alpha_o)\sin\beta + 3(a_o\cos\alpha_o)^2\cos\beta + 4(a_o\cos\alpha_o)(b_o\sin\alpha_o)\sin\beta - 2(b_o\sin\alpha_o)^2\cos\beta + \cdots$$
$$= \overset{\pm}{\Sigma} A_o\cos(\beta \pm \alpha_o)] \quad \}$$

$$+ \Sigma R^{o}\{[\cos\beta - 3(a_o\cos\alpha_o)\cos\beta - 2(b_o\sin\alpha_o)\sin\beta + 6(a_o\cos\alpha_o)^2\cos\beta + 6(a_o\cos\alpha_o)(b_o\sin\alpha_o)\sin\beta - 2(b_o\sin\alpha_o)^2\cos\beta + \cdots$$
$$= \overset{\pm}{\Sigma} B_o\cos(\beta \pm \alpha_o)] \times [-2a_1\cos\alpha_1]\}$$

$$+ \Sigma R^{o}\{[\sin\beta - 2(a_o\cos\alpha_o)\sin\beta + 2(b_o\sin\alpha_o)\cos\beta + 3(a_o\cos\alpha_o)^2\sin\beta - 4(a_o\cos\alpha_o)(b_o\sin\alpha_o)\cos\beta - 2(b_o\sin\alpha_o)^2\sin\beta + \cdots$$
$$= \overset{\pm}{\Sigma} A_o\cos(\beta \pm \alpha_o)] \times [-2b_1\sin\alpha_1]\}$$

$$+ \cdots$$

Let $a_o a_o b_o$ denote the coefficient of the argument α_o in the function $(a_o\cos\alpha_o)^2(b_o\sin\alpha_o)$ and similarly with other terms; then

$$\overset{\pm}{\Sigma} A_o = 1 - a_o \pm b_o + \frac{3}{2}a_o a_o \mp 2a_o b_o - b_o b_o + \cdots \qquad \overset{\pm}{\Sigma} B_o = 1 - \frac{3}{2}a_o \pm b_o + 3a_o a_o \mp 3a_o b_o - b_o b_o + \cdots$$

and the disturbing function can be written

$$R = \Sigma R^{o}_{1}(\overset{\pm}{\Sigma} A_o)\cos(\beta_1 \pm \alpha_o) + \Sigma R^{o}_{o}\{(\overset{\pm}{\Sigma} A_o)\cos(\beta_o \pm \alpha_o) + [\overset{\pm}{\Sigma} B_o\cos(\beta_o \pm \alpha_o)](-2a_1\cos\alpha_1) + [\overset{\pm}{\Sigma} A_o\sin(\beta_o \pm \alpha_o)](-2b_1\sin\alpha_1) + \cdots \quad \}.$$

In these expressions all the upper and all the lower signs are to be taken together, and the terms depending on the square of the mass of the disturbing planet have not been written down. There is obviously not the slightest difficulty in at once writing down every term to any desired order of the disturbing forces. When β is an argument which is independent of the angular distances between the Moon and disturbing body, the corresponding coefficient b must be put equal to zero in the coefficients ΣA_0 and ΣB_0. This is the form of the disturbing function which I have employed in my investigations on the terms of long period in the motion of the Moon; and it is particularly convenient, from the portions depending on the solar perturbations being kept distinct from that depending on the disturbing planet, so that they will be identical for every term of this kind in the theory of the Moon.

The first portion of this expression is the development of the disturbing function which has been denoted by R_1, and which represents the direct action of the planet upon the motion of the Moon, and upon the perturbations in this motion arising from the disturbing action of the Sun. It is this portion alone of the above expression which has been considered by Delaunay. The first term in the second portion is the expression of the part of the disturbing function due to the disturbing action of the Sun, and from which are derived the ordinary perturbations of the lunar theory. The second and third terms of this second part arise from substituting the complete perturbations of the Moon in the value of the ordinary disturbing function representing the action of the Sun upon the motion of the Moon. They really represent the portion of the disturbing function which expresses the action of the Sun upon the perturbations produced by the disturbing action of the planet upon the motion of the Moon. They yield the new class of terms to which attention has been directed.

The theory of the variation of parameters gives the following equation for determining the terms of long period in the mean motion of the Moon:—

$$\frac{d^2\delta n}{dt^2} = -3an^2\left(\frac{dR}{dE}\right),$$

where a is the mean distance, and E the longitude of the epoch. Let f be the arithmetical coefficient of E in the argument which has been denoted by β; then

$$\left(\frac{dR}{dE}\right) = \Sigma\,(-f)\,R\sin\beta.$$

The complete development of this expression can be obtained from that of R by writing $\beta - \frac{\pi}{2}$ for the arbitrary argument β. It will be therefore

$$\left(\frac{dR}{dE}\right) = \Sigma(-f).R^{o}_{1}(\overset{\pm}{\Sigma}A_o)\sin(\beta_1 \pm \alpha_o) + \Sigma(-f)\,R^{o}_{o}\{(\overset{\pm}{\Sigma}A_o)\sin(\beta_o \pm \alpha_o)$$

$$+ [\overset{\pm}{\Sigma}B_o \sin(\beta_o \pm \alpha_o)](-2a_1 \cos\alpha_1) + [\overset{\pm}{\Sigma}A_o \cos(\beta_o \pm \alpha_o)](-2b_1 \sin\alpha_1) + \cdots \}$$

It remains to be shown that the second portion of this expression will give rise to terms of long period with the same argument as those which can be obtained from the first portion, and which have alone been considered by Delaunay.

Let $Z \cos\zeta$ denote the complete value in $r\delta \frac{I}{r}$ of the term of long period which is derived from the first part of the above expression, and $Z^1 \sin\zeta$ its complete value in δv. These will then form particular forms of the general terms $a_1 \cos\alpha_1$ and $b_1 \sin\alpha_1$. The function which forms the coefficient of the term $(-2b_1 \sin\alpha_1)$ will have a constant portion of the order of the square and cube of the disturbing force of the Sun, and consisting of the coefficient of every pair of arguments in the series of arguments expressed by β_o and α_o which make $\beta_o = \alpha_o$.

This constant portion will be of the order m^4, and may be denoted by the term $\Sigma(R_o^o)_x(\Sigma A_o)_x$. Substituting these two values, then, as far as the term with the argument ζ, there will exist the term

$$\left(\frac{dR}{dE}\right) = \{\Sigma(R^o_o)_x \cdot (\Sigma A_o)_x\}(-2Z^1).\sin\zeta.$$

In a similar manner other terms will arise from this second part of the expression having the argument ζ and with coefficients containing Z and Z^1 multiplied by quantities of the order of the square and cube of the disturbing force of the Sun. Now Z and Z^1 are of the order of the disturbing force of the planet. This new class of terms must be described, therefore, as arising from the product of the square and cube of the disturbing force of the Sun with the disturbing force of the planet.

I think it is unquestionable that it is to these terms that Hansen refers in the paragraph which I have quoted from his memoir, and that it is from these terms that Hansen has derived the large value of the coefficient of the term of long period with the argument $(8g'' - 13g')$. For it is to these terms, and to these terms alone, that Hansen's words naturally apply. As these terms have been entirely neglected by Delaunay, this view would at once afford a possible explanation of the discrepancy between Hansen and Delaunay, and show at once why Hansen would not admit the correctness of the conclusions which were obtained by Delaunay. For these terms, depending on the ordinary perturbations arising from the direct action of *Venus* multiplied by the second power of the disturbing force of the Sun, will appear in the perturbations of the mean motion divided by the square of the very small coefficient of the time. They can very easily be far greater than the perturbations arising from

the direct action of *Venus*, which have been alone considered by Delaunay (the indirect effect of *Venus* through the agency of the perturbations of the Earth being disregarded). I think, therefore, it is almost certain that it is in this way that the discrepancy between Hansen and Delaunay has arisen.

It does not necessarily follow that the theoretical results which were obtained by Hansen are necessarily correct, although I have no doubt that it was from this second class of terms that Hansen derived the large value of the coefficient of the doubtful *Venus* term, and that consequently Delaunay's investigations do not really invalidate Hansen's conclusions. From his own remarks, it is evident that Hansen found very great difficulty in determining the theoretical value of these terms, a work of immense labour and difficulty by any of the ordinary methods. It is very possible that both of his results may have been vitiated by some error, just as one of the two conflicting results must have been wrong. The computations being extremely long, it is not unlikely that some error may have crept in; and it must be remembered that Delaunay, by neglecting these terms, has omitted the most difficult portion of the computation. It may be that these terms are altogether insensible. It is still more probable that the largest portion of them are mutually destructive, and in this manner vanish. I would urge merely that, until they have been shown to be insensible or to disappear, the theory of these two terms of long period must be considered to be imperfect.

These terms are of the third and fourth order of the disturbing forces, and it may be that it is possible to show by some established analytical principle, such as the invariability of the semi-axis major, that these terms must be mutually destructive. It is possible that Professor Newcomb may have investigated this point in his unpublished researches, and have thus shown that these terms do not affect the terms of long period in the lunar theory. I should be glad to learn whether this is so, for the past history of the lunar theory shows more than one instance where such principles have been overlooked, or else erroneously imagined to exist. To be sure that these terms may be safely neglected when determining the value of the coefficients of terms of long period similar to Hansen's, it must be shown that they vanish as far as the order m^7. To effect this by a direct computation would be a difficult and lengthy task. To show that they may be neglected in the case of all the terms of long period to which I have directed attention, it would be necessary to prove that they vanish to the order m^{10}. To effect this by direct computation would be an enormous work, and one which could not be carried out without pushing the lunar theory to a higher order than has ever yet been done.

I have endeavoured, therefore, to determine the value of these terms by a different method. In many portions of the lunar theory there arise different classes of important terms which in

the end destroy each other and so vanish. By developing the disturbing function in a general form it is possible, by an application of the method of the variation of arbitrary constants, to ascertain what terms destroy each other without its being necessary to determine the value of the terms in question. Thus if R be a function of the form $r \begin{matrix}\sin\\ \cos\end{matrix} \beta$, where r is constant and only β variable, as far as constant terms the function

$$\mathrm{R}\int \mathrm{R}\, dt = 0.$$

It can be further shown that in this expression every term is multiplied by the coefficient of the time in its argument. By similar considerations to these the work can be much reduced.

To show that the class of terms which have been referred to vanish to the order m^{10}, it would be necessary to show that they are mutually destructive to the fifth power of the disturbing forces, whereas no general development of the theory of the variation of constants seems to have been pushed beyond the terms of the second power of the disturbing forces. By a transformation I have pushed the development as far as the third power of the disturbing forces, and in part, though at present not completely, to the fourth power.

By this means it can be shown that the terms of the order m^4 and m^5 vanish from the perturbations in the mean motion, or before integration are multiplied by the small coefficient of the time in their argument. The same is true of most if not all of the terms arising from the second power of the disturbing force of the Sun and of the order m^6 and upwards. The same does not appear to hold of the third power of the disturbing force of the Sun or of the fourth power of the entire disturbing forces. I have not yet completed this last portion, but terms of the order m^6 appear to actually remain undestroyed. This result is confirmatory of the results obtained by direct computation in a very different manner.

If, then, there remain terms of the order m^6, it is quite possible that Hansen's theoretical values of the two terms of long period arising from *Venus* may be accurate.

On the Telescopic Observations of the Transit of Venus 1874, *made in the Expedition of the British Government, and on the Conclusions to be deduced from those Observations.* By E. T. Stone, M.A., F.R.S., Her Majesty's Astronomer, Cape of Good Hope.

I received by the last mail a copy of the Parliamentary Report whose title heads this Paper. This report contains the details of the observations of contact made by the observers of the British

Expeditions and the deduction of a value of the mean solar parallax (8″·76). I have examined the observations with great care, and discussed them in accordance with my own views. The result to which I have been led differs most materially from the value 8″·76 obtained in the Report, and I have therefore felt it my duty to lay my views before astronomers for their consideration. I presume that, with the evidence of these British observations, as well as that of the French observations before us, we need no longer insist upon the fact that the contacts in a transit of *Venus* are not mere geometrical contacts, but that when well seen in a fairly illuminated field, and when attention is directed to the point, the connection between the apparent limbs of *Venus* and the Sun is formed and broken gradually. The exact name which different observers may please to give to the phenomena observed matters but little, provided we can identify the observations which have taken place with the same angular separation of the limbs. The choice of assumed difference of semi-diameters applicable to the observed contacts is, as I have always maintained, the one difficulty in obtaining a very accurate value of the solar parallax from discussions of contact observations of the limbs of *Venus* and the Sun, and this same difficulty is presented in the methods of Halley and Delisle. The whole strength of the method for the determination of the solar parallax rests upon the assumption that there are phases of the contact which can be recognised by the observers as distinctive, and which at all the stations take place with the same angular separation of the limbs. It is not of course assumed that this is absolutely true; but the accuracy with which this assumption can be made in any class of observations, under the actual circumstances of observation, combined with the magnitudes of the parallax-factors, alone limits the accuracy attainable in the deduced value of the solar parallax. It is useless, therefore, except as a check on the principal phase, to record times corresponding to any but well-marked phases; and all time records corresponding to "limbs well separated," "bright light between the limbs," and such like, except as an indication of the time of last appearance of some disturbance previously seen, are without value for our present purpose.

I see no reason for abandoning my old views that the two phases of greatest value are the so-called apparent contacts and the so-called real contacts, of which the latter is the more important. By the real contact I merely mean the phase at which the observer first perceives the formation or disappearance of any permanent connection between the limbs of *Venus* and the Sun.

When we come to examine the observations made in 1874 we find that the apparent contacts have not been observed at the Sandwich Islands, one of the principal stations. It would almost appear from Mr. Nichol's remarks, and those of Capt. Tupman, that the measures of the cusps were run too close to the time of

contact to allow of any successful attempt to observe the apparent contacts. I am therefore compelled to restrict myself entirely in the present Paper to the real contact; but I shall divide them into two classes: the first corresponding to a disturbance of a very slight character, which can only, in my opinion, be seen under the conditions of a bright field and fair definition; and the second and more important phase when this shadow, or black drop, or haze, first assumes, or last appears of, a well-marked and permanent character. It will be seen, from the few attempts at the first phase, that the change of distance of centres is sufficiently great to prevent any confusion arising between these two kinds of observations, and the principal phase is well separated from the apparent contacts.

Sandwich Islands.

The following is Mr. Johnson's account of his observation at Waimea:—

	h	m	s	
{Cusps / Indefinite light} beginning to appear to meet .	7	17	57·8	G.S.T.
Meeting of cusps appeared to take place, and light clearing. I am inclined to think the actual contact took place between $7^h\ 17^m\ 57^s{\cdot}8$ and $7^h\ 18^m\ 7^s{\cdot}1$	7	18	7·1	
A distinct band of light between edge of *Venus* and the Sun's limb	7	18	12·0	

I have taken this observation under consideration, first, because it is preceded and followed by remarks showing that the time of total disappearance of the connection could not differ much from the time recorded, viz. $7^h\ 18^m\ 7^s{\cdot}1$. It is, however, probable that a mean between the two observations, $7^h\ 17^m\ 57^s{\cdot}8$ and $7^h\ 18^m\ 7^s{\cdot}1$, might be the true time of disappearance of the ligament.

Next, referring to the Honolulu observations, we have Capt. Tupman's observation:—

	h	m	s	
There was no *black* drop nor ligament. The planet was perfectly circular, and nothing whatever disturbed the sharpness of its outline at the place of contact. The band of light gradually brightened, and as Mr. Clapp said "twenty" it was sufficiently bright to induce me to consider the contact as established. My impression *afterwards* was that even at this time the Sun's limb was not yet perfectly distinct, and that some shadow remained on the light, but nothing was recorded after the "twenty." At that time the general appearance was similar to the model a second or two after contact	7	17	59·8	G.S.T.

Lieut. Noble's observation:—

There was no black drop, no ligament, but a rough dark shade, which gradually faded off to a thin tint corresponding to the phenomena I had observed in the model. This, instead of being nearly instantaneous, as the model generally showed, extended over some 20 seconds. The thin tint is that recorded, and I consider this the time of contact. I am certain it could not have been later than this . . . . . h 7 m 18 s 0·2 G.S.T.

Capt. Cator's observation (with diagram) 7 18 2·2

Lieut. Noble also states that at $7^h\ 18^m\ 20^s{\cdot}2$ the contact was long passed.

Now, we have here four observations of the real contacts. They differ amongst themselves rather more than could be wished, but not more, I believe, than such observations must fairly be expected to differ. There would be a better appearance of agreement if we took for Johnson's time the mean as before mentioned; but it appears to me better to take the results as they stand, and we have for real contact at the Sandwich Islands—

	Greenwich Sidereal Time. h m s	Local Tabular Distance of Centres. ′ ″
Capt. Tupman	7 17 59·8	15 37·58
Lieut. Noble	7 18 0·2	15 37·57
Mr. Johnson	7 18 7·1	15 37·35
Capt. Cator	7 18 2·2	15 37·40
Mean	7 18 2·3	15 37·48

Then, if $x =$ a constant for Ingress observations of real contact, $\delta\pi =$ connection to the assumed parallax $8''{\cdot}95$, and $\delta\pi = 8{\cdot}95 \times \frac{y}{100}$, we have for the real contact equation

$$x + 0{\cdot}2280\,y = 15'\ 37''{\cdot}48 \qquad 4 \text{ observations.}$$

It will be seen that the discordance in the estimation of the contact does not differ for any observer more than $0''{\cdot}13$ from the mean, a small quantity to be measured on the Sun's edge.

Rodriguez.

The real contact observations made by Commander Wharton and Lieut. Neate at this station are both clearly marked as the time of last appearance of haze or ligament.

Commander Wharton says :—

	h	m	s	
Directly after, a blackish haze was discernible joining the planet and the continuation of the Sun's limb; but from the agitation of the limb (the morning being cloudy) this haze was not distinctly defined and did not seem to change colour before disappearance. The moment of total disappearance of haze was tolerably defined, but I could not be certain to two or three seconds. The band of haze did not narrow at all before it broke that I could see. The time recorded is of total disappearance of haze as nearly as could be guessed	7	39	31·0	G.S.T.

Lieut. Neate :—

The ligament then gradually changed in colour (but not in breadth) getting lighter till it disappeared apparently suddenly, when the light between the limbs was uninterrupted . .	7	39	27·2	

Lieut. Hoggan :—

The ligament appeared to be decreasing in width very perceptibly	7	39	33·1	
It had faded away, and light appeared all round the planet's disk	7	39	48·6	

The first recorded time is early, the last would appear, from the use of the past tense, too late. A mean time, or $7^h\ 39^m\ 41^s{\cdot}85$, cannot be much in error and appears preferable to the use of either of these times. I may mention that, had I used the earlier time as that of last appearance, it would have given me a very slightly larger value of the solar parallax. Thus we have for real contacts at this station :—

	Greenwich Sidereal Time. h	m	s	Local Tabular Dist. of Centres. ′	″
Com. Wharton	7	39	31·0	15	37·88
Lieut. Neate	7	39	27·2	15	38·00
Lieut. Hoggan	7	39	41·9	15	37·58
Mean	7	39	33·4	15	37·82

Real contact equation $x - 0{\cdot}1947\,y = 15'\ 37''{\cdot}82$ 3 obs.

At Kerguelen we have observations of the disappearance of the connection or real contacts by three observers.

Lieut. Corbet :—

	h	m	s	
At this moment there was no more shade between the two limbs; it disappeared	7	40	33·2	G.S.T.

Lieut. Coke :—

The film disappeared	7	40	49·6	

Lieut. Goodridge:—

	h	m	s	
All appearance of shadow totally disappeared .	7	40	43·6	G.S.T.

Lieut. Corbet's time appears decidedly too early; but, on account of the difficulty of such observations, it does not appear desirable to assume too small a probable error, and I take the mean. It will be seen on consideration that if I had given less weight to Lieut. Corbet's observation, the value of the solar parallax deduced would have been greater. We have, then, for this station:—

	Greenwich Sidereal Time. h	m	s	Local Tabular Dist. of Centres. ′	″
Lieut. Corbet	7	40	33·2	15	38·29
Lieut. Coke	7	40	49·6	15	37·81
Lieut. Goodridge	7	40	43·6	15	37·93
Mean	7	40	42·1	15	38·01

and the equation of real contacts is

$$x - 0{\cdot}2150y = 15'\ 38''{\cdot}01, \qquad 3 \text{ obs.}$$

There remains the ingress observed by Major Palmer at Burnham. He says:—

	h	m	s	
At . . . . . as far as could be judged, the ligament seemed to undergo a change in depth of colour; but clouds prevented me from seeing whether any streak of light connecting cusps played across it.	7	29	38·1	G.S.T.
My last sight of the planet was at . . . . at which moment I *thought* I saw a very slight wavering streak of light, *though the cusps* (now very close) *had not fairly met.*	7	29	41·3	
I judged that it wanted about five seconds to complete separation of limbs. I counted the clock ticks for five seconds, and gave a signal for an estimated contact at	7	29	46·3	
The Sun did not appear again until about . .	7	43		

On account of cloud the absolute disappearance of the ligament at this station is only an estimation, but it was not gone at $7^h\ 29^m\ 41^s{\cdot}3$, which is important. Major Palmer's estimate places its disappearance at 5^s later, but such an estimation made during a glimpse through cloud could only be a rough one. I wish, however, to show that I am not straining the evidence in favour of any particular value of the solar parallax, and I will accept it as an observation, and work up the Ingress observations both including and excluding it.

The real contact equation derived from this observation is

$$x - 0{\cdot}0140\ y = 15'\ 38''{\cdot}07.$$

It will be seen that, on account of the small factor of y, this observation is not of much value for the determination of the solar parallax.

First, excluding the Burnham observation, the three equations for Ingress are

$$\sqrt{4}\,(x+0\cdot2280\,y) = \sqrt{4}\times0\cdot48,$$
$$\sqrt{3}\,(x-0\cdot1940\,y) = \sqrt{3}\times0\cdot82,$$
$$\sqrt{3}\,(x-0\cdot2150\,y) = \sqrt{3}\times1\cdot01,$$

which lead to

$$x-0\cdot0315\,y = +0\cdot741,$$
$$x-1\cdot4586\,y = +2\cdot193;$$

whence

$$x = +0''\cdot71,\ y = -1\cdot017,\ \text{and solar parallax} = 8''\cdot859.$$

These values give the following residual errors :—

Obs.—Comp.	
$+0''\cdot00$	Sandwich Islands.
$-0\cdot09$	Rodriguez.
$+0\cdot08$	Kerguelen.

Next, including the Burnham observation, we have, to find x and y, the equations

$$x-0\cdot0299\,y = +0\cdot771,$$
$$x-1\cdot3971\,y = +2\cdot145;$$

whence

$$x = +0''\cdot741,\ y = -1\cdot005,\ \text{and solar parallax} = 8''\cdot860.$$

The residual errors are now

$-0''\cdot03$	Sandwich Islands.
$+0\cdot31$	Burnham.
$-0\cdot12$	Rodriguez.
$+0\cdot05$	Kerguelen.

It would appear that Major Palmer's estimation is about 10^s too early, a result which can surprise no one. The general result from the Ingress observations is therefore

$$\pi = 8''\cdot860 \pm 0\cdot136 \times e,$$

where e is the probable error estimated in arc between the difference of the tabular distance of the centres corresponding to the time given by an observer and that which corresponds to the mean phase adopted at all the stations for Ingress. I

think e is certainly not greater than $0''.3$, and that the probable error of the above result cannot exceed $0''.04$.

Anyone who will examine my work will find that I have not selected the observations with a view to securing the largest possible value of the solar parallax. I have boldly taken the mean results of all attempts to observe the same phase, and I have done this on principle, although the observations may appear to differ somewhat wildly amongst themselves. For instance, I have taken for the time of the last appearance of the ligament at Kerguelen's Island the mean of the three results by Corbet, Coke, and Goodridge, although Corbet's time is 16^s earlier than that given by Coke; but in this transit of 1874 the velocity of separation of the limbs was so small that an error of 16^s is equivalent to less than an error of half a second of arc in the separation of the limbs, and this is certainly not an impossible quantity for the disagreement of two measures made upon the Sun's limb. As we have, however, the positive evidence of two observers that some connection between the limbs did exist for more than 10^s after the time given by Corbet, we might no doubt legitimately assume that all connection between the limbs of *Venus* and the Sun had not ceased at the time indicated by Corbet; but in my opinion we could not therefore legitimately reject his observation. It must be remembered that when we attempt to draw a value of the solar parallax from contact observations, we have to make an assumption that all attempts to observe the same phase are made with the same angular separation of the limbs; and it is certainly more probable that such an assumption will be satisfied when mean values are adopted rather than extreme ones. The discordances which exist amongst the observers at the same station are independent of any assumed value of parallax, and are therefore valuable as an indication of the errors which may be expected when we assume that the contacts for all stations take place with a mean distance between the centres; and we cannot fairly expect the discordances which arise from this assumption to be quite as small as those which exist among observations of the same phase made at a single station. I have been guided by this consideration in the rather liberal allowance which I have made for the probable error of the result.

The only cases in which I consider that I have exercised any arbitrary selection of the materials are:—

(1) The above case of Corbet (I might have given the observation less weight, or rejected it);
(2) The case of Johnson (I might have taken the mean value as before indicated);
(3) The case of Hoggan (in which I have taken a mean value).

But none of the alternatives to the selection which I have adopted can change the value of the solar parallax more than

0″·02, and I believe that I have had good reasons for the selections made.

It will of course be easy for anyone to vary the solution within small limits by slight changes such as those indicated, but I believe that no important changes in the value 8″·86 is possible without some clearly erroneous selection of a phase attempted to be caught by the observer. In the original solution contained in the Report an assumption has been made that what I have taken to be real contacts at the Sandwich Islands are some phase sensibly preceding the total disappearance of the connection between the limbs of *Venus* and the Sun; whilst some remarks made by the observers at different intervals after the total disappearance of any connection are supposed to be the real contact observations, and are marked "phase γ." These remarks, I have no doubt, were merely made by the observers to check the reality of the disappearance of any connection; and as they have no distinct relation to a definite separation of the limbs, they are only useful as checks upon the principal phase. It is this mistake which is the principal and almost sole cause of the difference between the value of the solar parallax obtained in the Parliamentary Report and that which I have obtained from a discussion of these Ingress observations. Before passing to the Egress observations, I would remark that the observations at Ingress and Egress are of a somewhat different character. At Ingress the observers are carefully watching the disappearance of something distinctly seen; whilst at egress they have to pick up, sometimes even through clouds, the first clear indications of a disturbance of the Sun's limb near the point of contact. There is therefore no great certainty, nor is there any necessity, that the phases at Ingress and Egress should exactly correspond. I fear, moreover, that attempts to estimate the degree of blackness of the ligament at Egress have led some of the observers to wait rather late before they have considered the contact as fairly established, and I doubt the expediency of these attempts to subdivide the phases.

The value of the Egress observations contained in the Parliamentary Report is also much diminished through the comparative failure, on account of cloud, of the observations at Kerguelen's Island; but the observations clearly point to a larger value of the solar parallax than that which I have deduced from the Ingress observations.

Mokattam and Suez.

We have four observations of the ligament well marked:—
Captain Browne says:—

> The shadow began to appear, and I tried to note the instant, but feared to interrupt my wife counting so near contact. Tried to note some

one instant, but could not, *the shadow was so gradual*. The Sun getting brighter, I saw a modified short ligament form, but did not see the edges form into sharp curves and creep apart, as sometimes in the model. Blackness gathered gradually, and at last came an instant when it seemed to get as black as the planet, and to have no light crossing it, which I noted by repeating the seconds then called by my wife . . . .. h m s 11 17 17·8 G.S.T

Mr. Hunter :—

Ligament now well marked . . . . . 11 17 24·08

At this station, then, we have the following observations :—

	Greenwich Sidereal Times. h m s	Local Tabular Dist. of Centres. ′ ″
Mr. Newton	11 17 14·3	15 41·34
Miss Newton	11 17 16·6	15 41·41
Capt. Browne	11 17 17·8	15 41·45
Mr. Hunter	11 17 24·1	15 41·62
Mean	11 17 18·2	15 41·455

The equation for this station for *ligament well marked* is

$$x + 0{\cdot}2020\,y = 15'\ 41''{\cdot}455 \qquad 4 \text{ obs.}$$

There is also a very early observation of a fine line at $11^h\ 16^m\ 39^s{\cdot}1$ with a tabular distance of centres equal to $15'\ 40''{\cdot}08$, and two geometrical contacts which correspond to a tabular distance $15'\ 44''{\cdot}54$ of the centres. It will be seen that the principal phase is quite separated from the two other phases, and that no confusion need be feared from the use of one for the other, except for the observations at Kerguelen, which were made through clouds. The observers, however, appear to have waited until the ligament was very strongly developed.

Thebes.

There is a little difficulty in interpreting the observations at this station, from the language of the observers; but the following three so-called "contacts" must be the same phase as that observed at Mokattam and Suez. This appears not only from the value of the tabular distances of centres, but also from the kind of contact represented in Colonel Campbell's figure :—

	Greenwich Sidereal Times. h m s	Local Tabular Dist. of Centres. ′ ″
Admiral Ommaney	11 16 46·8	15 41·43
Mrs. Campbell	11 16 51·0	15 41·56
Col. Campbell	11 16 53·3	15 41·64

There is an early observation of Mrs. Campbell's at $11^h\ 15^m\ 44^s{\cdot}3$, corresponding to a tabular distance $15'\ 39''{\cdot}31$ and called an appearance of shadow, and also one at $11^h\ 16^m\ 29^s{\cdot}5$, with a tabular distance $15'\ 40''{\cdot}84$ and called a small black drop. The equation for this station for ligament very well marked is

$$x + 0{\cdot}1910\,y = 15'\ 41''{\cdot}543 \qquad 3 \text{ obs.}$$

Rodriguez.

We have here two times recorded near the real contact so close together that some doubt may exist as to which should be taken. I will exhibit the results on both suppositions, and it will be found that the resulting values of the solar parallax do not sensibly differ.

Lieut. Neate :—

	h	m	s	
The first phenomenon noticed was the formation of a brown haze between the two limbs, and in spite of the excessive boiling this formation appeared instantaneously, and in appearance it closely resembled what I had seen at ingress in the second noted phenomenon	11	8	39·7	G.S.T.

Lieut. Hoggan :—

The light was not entirely interrupted at . .	11	8	41·4	
Light no longer visible between the limbs of the Sun and *Venus*, being interrupted by a band of about one-eighth of the diameter, and the same degree of blackness as the planet	11	8	46·4	

Commander Wharton :—

The brown haze appeared quickly, though not well defined at its edges as a ligament; it was of considerable breadth. Time recorded is of first appearance	11	8	41·7	
The haze or ligament darkened perceptibly. It is recorded as first appearance of a black hazy drop, the time of which was not very distinctly marked	11	8	50·3	

There are also observations of a very early phase by Lieut. Hoggan, at $11^h\ 8^m\ 8^s{\cdot}5$ G.S.T., of a slight distortion of the planet's limb which did not remain steady, corresponding to a tabular distance $15'\ 40''{\cdot}30$, and three apparent contacts corresponding to a mean distance $15'\ 43''{\cdot}307$. Our observations are then :—

1st Set.	Greenwich Sidereal Time. h	m	s	Local Tabular Dist. of Centres. ′	″
Lieut. Neate	11	8	39·7	15	41·27
Lieut. Hoggan	11	8	41·4	15	41·37
Comm. Wharton	11	8	41·7	15	41·34
Mean				15	41·327

and the equation for this phase is

$$x + 0{\cdot}0270\,y = 15'\ 41''{\cdot}327 \qquad 3 \text{ obs.}$$

2nd Set.	h	m	s	′	″
Lieut. Hoggan	11	8	46·4	15	41·53
Com. Wharton	11	8	50·3	15	41·62
Mean				15	41·575

and the equation for ligament strongly marked is

$$x + 0{\cdot}0270\,y = 15'\ 41''{\cdot}575 \qquad 2 \text{ obs.}$$

Kerguelen's Island.

Rev. W. Sidgreaves:—

	h	m	s	
Sun nearly obscured. Circular contact. Nothing seen before this. Very uncertain observation; image scarcely visible through the cloud . .	11	1	58·1	G.S.T.

Rev. S. J. Perry:—

A good geometrical contact. There was no appearance of black drop before contact, but this might have been due to the obscuring clouds .	11	2	15·8	

Lieut. Goodridge:—

Passing clouds. At this time a few lines or slight shadow appeared between the two limbs, just visible through cloud	11	1	39·9	
At this time the shadow gradually darkened until the black drop assumed a solid appearance of the same density as the planet. A dark cloud came over, and I lost all further view . .	11	1	53·4	

We have, then, the following observations at this station:—

	Greenwich Sidereal Times. h	m	s	Local Tabular Dist. of Centres. ′	″	
Lieut. Goodridge	11	1	39·9	15	40·61	Shadow.
Lieut. Goodridge	11	1	53·4	15	41·03	Solid black drop.
Rev. W. Sidgreaves	11	1	58·1	15	41·14	Contact.
Rev. S. J. Perry	11	2	15·8	15	41·68	Contact.

Whatever interpretation we may be pleased to put upon these observations, there is no doubt that a well-marked connection was established at $11^h\ 1^m\ 55^s{\cdot}7$ G.S.T. This is shown by both Sidgreave's and Goodridge's observations. It is impossible to believe that the time given can be much too early, and therefore

the corresponding distance of centres 15′ 41″·08 cannot be much too small.

I will exhibit the values of the solar parallax which would be required to satisfy the equations. (1) Taking the first set of observations at Rodriguez, we have the following equations :—

$$\sqrt{4}\,(x+0{\cdot}2020\,y) = \sqrt{4}\times 0{\cdot}455,$$
$$\sqrt{3}\,(x+0{\cdot}1910\,y) = \sqrt{3}\times 0{\cdot}543,$$
$$\sqrt{3}\,(x+0{\cdot}0270\,y) = \sqrt{3}\times 0{\cdot}327,$$
$$\sqrt{2}\,(x-0{\cdot}1000\,y) = \sqrt{2}\times 0{\cdot}080.$$

These lead to the following equations for the determination of x and y :—

$$x+0{\cdot}1052\,y = 0{\cdot}3825,$$
$$x+0{\cdot}2336\,y = 0{\cdot}5461;$$

whence

$$x = 0''{\cdot}249,\ y = +1{\cdot}274,\ \text{and parallax} = 9''{\cdot}06.$$

(2) Taking the second set at Rodriguez, we have

$$\sqrt{4}\,(x+0{\cdot}2020\,y) = \sqrt{4}\times 0{\cdot}455,$$
$$\sqrt{3}\,(x+0{\cdot}1910\,y) = \sqrt{3}\times 0{\cdot}543,$$
$$\sqrt{2}\,(x+0{\cdot}0270\,y) = \sqrt{2}\times 0{\cdot}575,$$
$$\sqrt{2}\,(x-0{\cdot}1000\,y) = \sqrt{2}\times 0{\cdot}080,$$

which give for the determination of x and y the equations

$$x+0{\cdot}1123\,y = 0{\cdot}4326,$$
$$x+0{\cdot}2381\,y = 0{\cdot}5618;$$

whence

$$x = +0''{\cdot}317,\ y = +1{\cdot}027,\ \text{and parallax} = 9''{\cdot}04.$$

One of the results—9″·04 or 9″·06, or their mean—would, in my opinion, have to be accepted as the best value deducible from these Egress observations, were it not that we are compelled to recognise that the observations at Kerguelen were caught up with great difficulty through clouds, and that we have a later observation of Perry's, which, although called a contact, is completely separated off from the apparent contacts at the other stations, and must therefore be accepted as an observation much nearer the time of so-called real contact; and we have already pointed out that the observations in Egypt have been made when the ligament or black drop has been very largely developed. I will next take Perry's observation of contact as that of ligament well marked, and use with it the second set at Rodriguez. This is a most extreme supposition, and would not be worth consideration were it not for the fact that the observations at Thebes, which are clearly not observations of apparent contact in the same sense as

those made at the other stations, are yet called "contacts," and are evidently the same phase as that observed at Mokattam and Suez, where it is clear that the times have been pushed to an extreme limit by the observers waiting for the ligament to assume a sufficient degree of blackness. These considerations do perhaps to some extent justify the use of Perry's observation to the exclusion of those of Sidgreaves and Goodridge, but such an exclusion is certainly not satisfactory. On this supposition we have

$$\sqrt{4}\,(x + 0{\cdot}2020\,y) = \sqrt{4} \times 0{\cdot}455,$$
$$\sqrt{3}\,(x + 0{\cdot}1910\,y) = \sqrt{3} \times 0{\cdot}543,$$
$$\sqrt{2}\,(x + 0{\cdot}0270\,y) = \sqrt{2} \times 0{\cdot}575,$$
$$\sqrt{1}\,(x - 0{\cdot}1000\,y) = \sqrt{1} \times 0{\cdot}680,$$

which lead to the equations

$$x + 0{\cdot}1335\,y = 0{\cdot}5279,$$
$$x + 0{\cdot}2128\,y = 0{\cdot}4807;$$

whence

$$x = +0''{\cdot}607,\ y = -0{\cdot}591,\ \text{and parallax} = 8''{\cdot}897.$$

This value, like the value $9''{\cdot}05$, must be regarded as resting upon an extreme assumption, but in an opposite direction to the former one. But, on account of the existence of clouds at Kerguelen and the impossibility of discriminating between the kinds of contact observed, we should perhaps combine at all the stations attempts to observe real contacts without any discrimination between the finer phenomena presented. But if we look at Goodridge's observation made at $11^h\ 1^m\ 39^s{\cdot}9$, we shall see that, although the limbs were only just visible through cloud, some decided connection was already established. I am hardly prepared, therefore, to consider that this observation can be fairly classed with such observations of a very early phase as that of Mrs. Campbell, which was made under the most favourable atmospheric circumstances. But as I wish to exhibit clearly the extreme limits within which the values of the solar parallax must be confined when deduced from any legitimate assumption, I will discuss these contact observations, both excluding and including the very early ones. It will be seen that the two solutions agree very closely.

The result of a discussion of the four very early observations by themselves in a separate class is not worth consideration when the circumstances under which Goodridge's observation was made are considered.

The following solution is based upon the mean of all attempts to observe real contacts, excluding only attempts at apparent contacts, which are separated at the stations at which

both are observed by more than a minute of time from the phase used, and whose results could not therefore, under any circumstances, be admitted in the equations of condition. The following are the equations on this assumption:—

$x + 0{\cdot}2020\,y =$	$0''{\cdot}18$	5 obs.
$x + 0{\cdot}1910\,y =$	$-0{\cdot}04$	5 obs.
$x + 0{\cdot}0270\,y =$	$0{\cdot}24$	6 obs.
$x - 0{\cdot}1000\,y =$	$0{\cdot}12$	4 obs.

The number of observations at the different stations are so nearly equal that we shall assume the weights to be equal, and we have

$$x + 0{\cdot}0800\,y = 0{\cdot}1250,$$
$$x + 0{\cdot}2750\,y = 0{\cdot}0725;$$

whence

$$x = 0''{\cdot}147,\ y = -0{\cdot}27, \text{ and parallax} = 8''{\cdot}93.$$

The residual errors are

Obs.—Comp.	
$+0''{\cdot}087$	Mokattam and Suez,
$-0{\cdot}135$	Thebes,
$+0{\cdot}100$	Rodriguez,
$-0{\cdot}054$	Kerguelen.

There remains only a solution obtained by combining all the observations from "ligament clearly seen" to so-called "contacts," but excluding the very early observations of shadows or interference lines, a phase which is only caught by a single observer at each station, and which should therefore be excluded from our equations of condition from this consideration alone. On this supposition our equations of condition are

$$\sqrt{4}\,(x + 0{\cdot}2020\,y) = \sqrt{4} \times 0{\cdot}455,$$
$$\sqrt{4}\,(x + 0{\cdot}1910\,y) = \sqrt{4} \times 0{\cdot}368,$$
$$\sqrt{5}\,(x + 0{\cdot}0270\,y) = \sqrt{5} \times 0{\cdot}426,$$
$$\sqrt{3}\,(x - 0\ 1000\,y) = \sqrt{3} \times 0{\cdot}283.$$

From these we get

$$x + 0{\cdot}0879\,y = 0{\cdot}3919,$$
$$x + 0{\cdot}2436\,y = 0{\cdot}4416;$$

whence

$$x = +0''{\cdot}364,\ y = 0{\cdot}319, \text{ and parallax} = 8''{\cdot}979.$$

The residual errors are

Obs.—Comp.	
"	
+0·027	Mokattam and Suez,
−0·057	Thebes,
+0·053	Rodriguez,
−0·049	Kerguelen.

These residual errors are very small, and this appears to be the only satisfactory way of obtaining any good *independent* value from these Egress observations. The value thus found is larger than we have reason to believe it should be, but the probable error of the determination is considerable. From these Egress observations, then we have,

$$\pi = 8''{\cdot}979 \pm 0{\cdot}279 \times e,$$

where e has a similar meaning to that in the Ingress observations.

It appears to me that these Egress observations cannot be satisfied in any way by a value less than 8″·88, and that the value 8″·98 is that which is derived from them by the most satisfactory *à priori* assumption of corresponding observations. Accepting, therefore, for the Ingress $8''{\cdot}860 \pm 0{\cdot}136\ e$, and for the Egress $8''{\cdot}979 \pm 0{\cdot}279\ e$, we have for the mean solar parallax

$$8''{\cdot}884 \pm 0{\cdot}123 \times e \qquad (5)$$

as the value deducible from these contact observations, a value which I believe absolutely negatives any value smaller than 8″·84 or larger than 8″·93.

My own earlier determinations are

	"		
(1) From observations of *Mars* in 1862	8·94	Published	1863.
(2) From parallactic inequality in Earth's motion	8·91	„	1867.
A value of small weight from its depending on the assumed value of the Moon's mass.			
(3) From parallactic inequality in Moon's motion	8·85	„	1867.
Deduced from 2075 observations near the times of maximum effect.			
(4) From a rediscussion of Transit of *Venus* Observations, 1769	8·91	„	1868.

These five determinations give me

$$\pi = 8''{\cdot}897 \pm 0''{\cdot}020.$$

Besides these I give for comparison with my results the following:—

Dr. Winnecke, from the *Mars* observations in 1862, deduced a value 8″·96. This was published in 1863.

Mr. Newcomb's discussion of the *Mars* observations in 1862, published in 1867, gave 8″·855. The smaller value deduced from these *Mars* observations by Mr. Newcomb depends chiefly on the use of the Santiago observations, where the inclination of the wire used was not determined. Mr. Newcomb has attempted to eliminate the errors thus introduced, but I think that the use of these Santiago observations is, under the circumstances, inadmissible.

Hansen's value, deduced from the parallactic inequality in the Moon's motion, is 8″·916. This value was first published in 1867. Hansen had intimated several years before that the parallactic inequality required an increase, but it is doubtful whether he was prepared at that time to throw the whole of this required increase upon an error in the assumed value of the solar parallax. There are also the determinations from the velocity of light which have lately been repeated by M. Cornu, and which, with the accepted value of the coefficient of aberration, lead to a value 8″·86. And there are the classical investigations of Le Verrier, in which, starting with an erroneous value of the ratio of the Sun's mass to that of the Earth deduced from Encke's value of the solar parallax, he not only showed from the theories of the planets themselves that the assumed value of the solar parallax was incorrect, but deduced a value 8″·88, which is entitled to great weight, and is in most satisfactory agreement with the values deduced more directly from observation. If we assume $\pi = 8''{\cdot}89$, the correction which this assumed value requires must be very small. This will correspond to a distance of the Earth from the Sun equal to 91,940,000 miles.

Royal Observatory, Cape of Good Hope,
1878, *January* 18.

On Certain Groups of Stars with Common Proper Motions.

By Prof. T. H. Safford.

It has been long noticed by those who have given attention to the subject of proper motion that there are many groups of stars which seem to have a common proper motion. This tendency has been called star-drift by Mr. Proctor; and the special instance which he has brought prominently forward is that of the group of five stars in *Ursa Major*, β, γ, δ, ε, ζ. The motion of these stars is nearly in the same direction, nearly opposed to that which results from the solar motion, but varies in amount from about 0″·09 to 0″·15 yearly.

Upon this subject future determinations will doubtless throw a great deal of light. I am inclined to think that the observations and calculations of the next few years will do much in this direction, as we can now combine good modern observations with ancient ones, not only of Bradley, Lalande, Piazzi, and Groombridge, but also of Struve, Pond, Bessel, and Argelander.

Of the 68 stars within 10° of the North Pole which are reckoned visible to the naked eye (either by Argelander, Heis, or Carrington) about one-half have proper motions whose determination can now be effected with some accuracy. I have already deduced them in a provisional manner, in order to be able to compute more exactly the secular variations and terms of higher orders of the whole motion; or, in other words, to be able to establish formulæ which shall nearly represent observation and be easily corrected.

There are several groups which have proper motions nearly alike in amount and direction.

The first group to which I shall call attention is composed of six stars, whose names (according to Argelander), B.A.C. Nos., rough positions for 1855, and annual proper motions here follow:—

Name.	No. B.A.C.	A.R. h m	Decl. ° ′	Δα. s	Δα cos δ. ″	Δδ. ″
36 *Cephei* H.	8026	22 55·4	83 33·2	+0·05	+0·08	+0·02
39 *Cephei* H.	8213	23 27·8	86 30·4	+0·085	+0·08	+0·01
Bradley 3187	8321	23 49·7	82 23·0	+0·03	+0·06	0·00
Bradley 74	225	0 41·6	82 55·1	+0·04	+0·07	−0·01
43 *Cephei* H. (2 *Ursæ Minoris*).	262	0 49·7	85 28·6	+0·068	+0·08	0·00
Bradley 95	273	0 52·8	86 22·2	+0·07	+0·06	−0·01

It will be seen that a mean proper motion of +0″·07 and 0″·00 is almost as exact for the individual stars as the proper motions can be depended upon, and that the slight deviations in declination indicate that the common motion of all these stars is nearly perpendicular to the equinoctial colure.

Polaris itself has a less proper motion in nearly the same direction; and the small star B.A.C. 240 which precedes it shows traces of a near relationship in its motion, as follows:—

Name.	No. B.A.C.	A.R. h m	Decl. ° ′	Δα. s	Δα cos δ. ″	Δδ. ″
Bradley 65	240	0 45·4	88 14·6	+0·11	+0·05	−0·02
Polaris	360	1 6·5	88 32·2	+0·11	+0·04	0·00

It seems, however, unlikely that the same value of Δδ will represent as fully as is desirable the observations of both stars. This point must be decided by a fuller study.

In the third group are contained three stars:—

Name.	No. B.A.C.	A.R. h m	Decl. ° ′	Δα. s	Δα cos δ. ″	Δδ. ″
Bradley 344	784	2 27·2	80 49·6	+0·02	+0·05	−0·08
Bradley 402	960	3 58·8	84 23·1	+0·04	+0·06	−0·09
Radcliffe 1311	...	4 41·0	85 45·2	+0·04	+0·04	−0·09

The parallelism of motion here is not quite so striking, but will need further observations either to prove or disprove.

Two other stars make a pretty sure pair, namely:—

Name.	No. B.A.C.	A.R. h m	Decl. ° ′	Δα. s	Δα cos δ. ″	Δδ. ″
Groombridge 1850	4070	11 57·4	86 23·4	−0·06	−0·05	+0·06
6 *Ursæ Minoris* B.	4165	12 14·4	88 30·2	−0·094	−0·04	+0·07

Still two others are quite doubtful, although agreeing in amount more nearly than the two stars β and ζ *Ursæ Majoris*:—

Name.	No. B.A.C.	A.R. h m	Decl. ° ′	Δα. s	Δα cos δ. ′	Δδ. ″
30 *Camelop.* H.	3528	10 13·0	83 17·6	−0·052	−0·09	+0·03
202 *Camelop.* B.	3906	11 21·4	81 58·5	−0·06	−0·13	+0·04

As to the dependence to be placed on these numbers, it is naturally not very large. They are, however, pretty fairly substantiated by the evidence, which in nearly all the cases is abundant. How they were discussed may be seen from the following table for the last star mentioned, giving the positions reduced to 1855:—

	h m s	° ′ ″	C−O. s	C−O. ″
Groombridge	11 21 28·9	81 58 28·1	−0·1	−0·1
Struve 1815	28·3	...	+0·2	...
Schwerd 1828	27·5	29·1	+0·2	−0·4
Radcliffe	26·8	28·3	−0·1	+1·1
Yarnall 1846·8	27·1	...	−0·5 (1 observation).	...
Carrington	26·4	30·1	−0·3	−0·3
H.C. 1865	25·5	...	0·0	...
Yarnall 1869	...	30·6	...	−0·2

These figures were obtained at second-hand in several cases, so I only wished to gain approximate positions and proper motions in order to calculate precessions and secular variations (the latter including terms depending on proper motion) with the needful precision. The larger proper motions in this region are mostly well known. The star 223 *Camelop.* Bode = B.A.C. 4982 is the chief exception to this statement. Its annual proper motion is about $+0^{s}\cdot10$ in A.R. and $-0''\cdot27$ in Declination, which, at 6°·9 from the Pole, amounts to rather more than $0''\cdot3$ in arc.

Williams College, Williamstown, Mass., U.S.A.
1878, January 12.

Ephemeris for Physical Observations of Jupiter, 1878. By A. Marth, Esq.

0^h G.M.T.	Angle of Position of ♃'s Axis.	Longitude of ♃'s Meridian directed to the Earth.	Diff.	Latitude of Earth above ♃'s Equator.	Latitude of Sun above ♃'s Equator.	Annual Parallax.	Equat. Diam.	Greatest Phase. Prec. side.	Corr. of Long.
1878.	°	°	°	°	°	"	"	"	°
Apr. 25	343·89	126·04		−0·74	−1·08	−11·28	38·71	0·374	+0·55
			4352·98						
30	343·74	159·02		·71	1·06	11·28	39·33	·380	·55
			3·05						
May 5	343·62	192·07		−0·68	−1·04	−11·20	39·96	·381	+0·55
			3·12						
10	343·52	225·19		·65	1·02	11·05	40·61	·376	·53
			3·19						
15	343·45	258·38		·63	1·00	10·83	41·27	·368	·51
			3·25						
20	343·41	291·63		·61	0·98	10·53	41·93	·353	·48
			3·32						
25	343·39	324·95		·59	·95	10·14	42·60	·333	·45
			3·39						
30	343·40	358·34		·58	·93	9·67	43·25	·307	·41
			3·45						
June 4	343·44	31·79		−0·56	−0·91	−9·13	43·89	·278	+0·36
			3·50						
9	343·51	65·29		·55	·89	8·51	44·51	·245	·32
			3·56						
14	343·60	98·85		·54	·87	7·81	45·10	·209	·27
			3·60						
19	343·71	132·45		·54	·85	7·04	45·65	·172	·22
			3·64						
24	343·85	166·09		·53	·82	6·21	46·15	·135	·17
			3·68						
29	344·02	199·77		·53	·80	5·31	46·60	·100	·12
			3·69						
July 4	344·20	233·46		−0·53	−0·78	−4·36	46·99	·068	+0·08
			3·71						
9	344·40	267·17		·54	·76	3·36	47·30	·041	·05
			3·71						
14	344·62	300·88		·54	·74	2·33	47·54	·020	·02
			3·71						
19	344·85	334·59		·55	·71	1·27	47·70	·006	+ ·01
			3·68						
24	345·08	8·27		·56	·69	−0·19	47·77	foll. side	·00
			3·64						
29	345·32	41·91		·57	·67	+0·88	47·76	·003	·00
			3·58						
Aug. 3	345·56	75·49		−0·58	−0·65	+1·95	47·66	·014	−0·02
			3·54						
8	345·79	109·03		·59	·62	3·00	47·47	·033	·04
			3·47						
13	346·02	142·50		·60	·60	4·02	47·21	·058	·07
			3·39						

0^h G.M.T.	Angle of Position of ♃'s Axis.	Longitude of ♃'s Meridian directed to the Earth.	Diff.	Latitude of Earth above ♃'s Equator.	Latitude of Sun above ♃'s Equator.	Annual Parallax.	Equat. Diam.	Greatest Phase. Prec. side.	Corr. of Long.
1878.	°	°	°	°	°	°	″	″	°
Aug. 18	346·23	175 89		·61	·58	4·99	46·87	·089	·11
			3·31						
23	346·42	209·20		·62	·55	5·91	46·47	·123	·15
			3·22						
28	346·59	242·42		·63	·53	6·77	46·00	·161	·20
			3·13						
Sept. 2	346·73	275·55		—0·64	—0·51	+7·57	45·49	·198	−0·25
			3·02						
7	346·84	308·57		·64	·49	8·30	44·93	·235	·30
			2·93						
12	346·93	341·50		·65	·46	8·95	44·33	·270	·35
			2·82						
17	346·98	14·32		·65	·44	9·52	43·71	·301	·40
			2·73						
22	347·01	47·05		·65	·42	10·02	43·08	·329	·44
			2·63						
27	347·00	79·68		·65	·39	10·43	42·43	·351	·47
			2·53						
Oct. 2	346·96	112·21		−0·65	−0·37	+10·75	41·77	·367	−0·50
			2·44						
7	346·88	144·65		·65	·35	11·00	41·12	·378	·53
			2·36						
12	346·78	177·01		·64	·33	11·17	40·48	·383	·54
			2·27						
17	346·64	209·28		·64	·30	11·26	39·85	·383	·55
			2·20						
22	346·48	241·48		·63	·28	11·28	39·23	·379	·55
			2·13						
27	346·29	273·61		·61	·26	11·23	38·63	·370	·55
			2·06						
Nov. 1	346·08	305·67		−0·60	−0·23	+11·11	38·05	·365	−0·54
			2·00						
6	345·84	337·67		·58	·21	10·93	37·50	·340	·52
			1·94						
11	345·59	9·61		·56	·19	10·68	36·97	·320	·50
			1·90						
16	345·31	41·51		·54	·16	10·38	36·47	·298	·47
			4351·86						
21	345·02	73·37		−0·52	−0·14	+10·03	36·00	0·275	−0·44

Assumed daily rate of rotation 870°·60. The "annual parallax" is the difference of the Jovicentric longitudes of the Sun and Earth, reckoned in the plane of *Jupiter's* equator. The last column gives the correction, which is to be applied to the "longitude of the meridian directed to the Earth," in order that it may refer to the meridian which bisects the illuminated disk. The inclinations γ and the ascending nodes Γ of the orbits of the satellites in reference to the plane of *Jupiter's* equator are the following, the nodes being reckoned from the descending node of the equator on the planet's orbit, or from the vernal equinox of *Jupiter's* northern hemisphere:—

	Sat. I.		Sat. II.		Sat. III.		Sat. IV.	
	γ_1	Γ_1	γ_2	Γ_2	γ_3	Γ_3	γ_4	Γ_4
1878.	°	°	°	°	°	°	°	°
Feb. 24	0·0097	30·8	0·4553	37·42	0·1942	271·14	0·3254	330·22
April 25	97	28·7	·4559	35·40	·1936	270·60	·3250	330·12
June 24	96	26·6	·4566	33·37	·1930	270·08	·3247	330·01
Aug. 23	96	24·4	·4573	31·35	·1924	269·58	·3245	329·90
Oct. 22	96	22·1	·4581	29·32	·1918	269·10	·3244	329·80
Dec. 21	0·0096	19 7	0·4589	27·29	0·1913	268·64	0·3245	329·70

Correction to the Ephemeris of the Satellites of Uranus.

On page 97 the values of the position angles p_2 are, by mistake, wrongly given. The correct values are as much above 10° as the printed ones are below 10°, or the true values are found by subtracting the printed ones from 20°. Thus

	°		°
For	8·13	*read*	11·87
,,	8·19	,,	11·81
*	*	*	*
,,	8·95	,,	11·05.

Observations of Occultations of Stars by the Moon, and of Phenomena of Jupiter's Satellites, made at the Royal Observatory, Greenwich, in the year 1877.

(*Communicated by the Astronomer Royal.*)

Occultations of Stars by the Moon.

Day of Obs.	Phenomenon.	Telescope.	Power.	Moon's Limb.	Mean Solar Time of Observation.	Observer.
1877.					h m s	
Jan. 30	Reapp. of 45 Leonis	Altaz.	100	Dark	9 15 50·3	A.D.
,,	Disapp. of ρ Leonis	,,	,,	Bright	10 51 19·4	,,
,,	Reapp. of ρ Leonis	,,	,,	Dark	11 57 2·8	,,
Feb. 26	Disapp. of Regulus	E. Eq.	140	,,	12 45 43·8	M.
,, (*a*)	,, ,,	S.E. Eq.	285	,,	12 45 44·0	W.C.
,,	,, ,,	Altaz.	100	,,	12 45 44·3	A.D.
Mar. 21	,, Piazzi V. 136	,,	,,	,,	10 20 31·9	T.
Nov. 20 (*b*)	,, 19 Tauri	S.E. Eq.	130	Bright	7 42 19·7	M.
,, (*c*)	,, 20 Tauri	,,	,,	,,	7 49 40·0	,,
,,	Reapp. of 17 Tauri	,,	,,	,,	8 10 31·1	,,
,,	,, 19 Tauri	,,	60	,,	8 39 10·9	,,
,, (*d*)	,, 20 Tauri	,,	,,	,,	8 55 55·5	,,

Phenomena of Jupiter's Satellites.

Day of Obs. 1877.	Satellite.	Phenomenon.	Telescope.	Power.	Mean Solar Time of Observation. h m s	Mean Solar Time from *N.A.* h m s	Observer.
May 7 (*e*)	II	Occ. reapp. bisection	S.E. Eq.	220	13 4 25·1		M.
„	II	„ last cont.	„	„	13 6 27·8		„
„ (*f*)	II	„ first cont.	E. Eq.	70	13 1 0·4	13 0	L.
„	II	„ bisection	„	„	13 2 36·5		„
„	II	„ last cont.	„	„	13 5 32·4		„
June 20 (*g*)	I	Tr. ing. last contact	„	140	10 48 36·7	10 50 .	T.
July 20	I	„ first contact	„	„	12 13 24·7		A.D.
„	I	„ last contact	„	„	12 16 34·2	12 16	„
24 (*h*)	II	Occ. disapp. last cont.	„	„	11 12 11·3	11 11	„
26 (*i*)	II	Tr. egr. bisection	„	„	9 2 38·0		B.D.
„	II	„ last contact	„	„	9 5 47·5	9 3	„
29 (*k*)	I	„ first contact	„	„	10 42 38·0		C.
„	I	„ bisection	„	„	10 43 37·8	10 45	„
„	I	„ last contact	„	„	10 45 7·6		„
30	I	Ecl. reapp. first seen	S.E. Eq.	285	8 56 32·1		M.
„	I	„ full brightness	„	„	8 56 57·1	8 56 37	„
„	I	„ full brightness	E. Eq.	140	8 57 0·9		R.
Sept. 5	II	Ecl. reappearance	„	„	7 14 1·2	7 14 36	J.B.

Notes.

(*a*) The star disappeared instantaneously; sky cloudy.
(*b*) The Moon's limb was exceedingly tremulous. The star was lost to view several times before reaching the limb, and finally disappeared in a hollow.
(*c*) The time of disappearance was very difficult to ascertain, owing to the glare of the Moon, which was sensibly full.
(*d*) The Moon's limb was fairly steady.
(*e*) *Jupiter* was very tremulous, and the limb badly defined.
(*f*) Image bad; the observation very unsatisfactory.
(*g*) The definition of the planet was bad, owing to light cloud.
(*h*) The observation unsatisfactory; a diffused light round *Jupiter*.
(*i*) *Jupiter* very tremulous, and the observation uncertain.
(*k*) Image bad.

The clear aperture of the object-glass of the S.E. Equatoreal is $12\frac{3}{4}$ inches, of the East Equatoreal 6·7 inches, and of the Altazimuth $3\frac{3}{4}$ inches.

The initials W.C., L., C., A.D., M., T., B.D., R., and J.B. are those of Mr. Christie, Mr. Lynn, Mr. Criswick. Mr. Downing, Mr. Maunder, Mr. Thackeray, Mr. Dennison, Mr. Robinson, and Mr. Bromley.

Royal Observatory, Greenwich,
1878, *February* 13.

On Mr. Stone's Note in the January Number of the Monthly Notices. By Richard A. Proctor, B.A., Hon. Fell. King's College, London.

Mr. Stone calls my attention to a Paper by Sir G. Airy, in vol. xxiv. of the *Monthly Notices*, showing that the part of the disk of the Sun seen through a refracting medium, causing an effect analogous to that of a convex lens, will not be of greater apparent brightness than the rest. He is right in presuming that I was unacquainted with Sir G. Airy's investigation when my paper was written; but not quite so in saying that the investigation covers the ground of my paper, if at least he correctly sums up its results as this, that "no refraction can cause a change in the apparent brightness of the surface visible." For in my paper I considered the possibility of reflexion as well as refraction, and of various conceivable combinations of both reflexion and refraction. In the article entitled "Notes on Brightness," referred to in my paper, I considered the effect of all possible combinations of reflexion and refraction, including, though not specially mentioning, the case of refraction through prisms, which some seem to suppose competent to increase the absolute apparent lustre of a surface. This cannot actually happen, though the relative lustre of an object and its surroundings may be so modified that the visibility of the surface may be greatly strengthened, or (as in the case of the solar prominences) an object rendered visible which was before undiscernible.

My remarks related entirely to intrinsic surface brightness. So that, as far as nebulæ are concerned, they only apply to cases where (whether by varieties of telescopic power or of distance) a nebula is neither so magnified that individual component stars are seen, nor so reduced in size as to seem a mere point. In the former case the individual stars necessarily appear brighter with increase of telescopic power, or diminution of distance; in the latter the nebula as a whole does so. It is hardly necessary to say that the satellites of *Mars*, appearing as mere points in the most powerful instruments yet made, cannot be used to illustrate the subject of apparent intrinsic brightness. I was so careful to draw a distinction between intrinsic brightness and quantity of light, that I cannot recognise the justice of the distinction drawn by Mr. Stone between what I said and what I probably meant. He asks why, if my statement (ending with the words that "quantity of light can of course be greatly increased") were true, the satellites of *Mars* could not as easily be seen with a 4-inch as with a 26-inch telescope; a question which actually contains its own answer, viz. that quantity of light is of course greatly increased when the larger telescope is used.*

* At the meeting when the present paper was read, it was suggested that Mr. Stone may have referred to the different dimensions of the diffraction disks

Unless, indeed, it be necessary to explain that the visibility of an object depends in some degree on the quantity of light we receive from it.

I am sorry, on the other hand, to have misinterpreted Mr. Stone's own remarks about gaseous nebulæ. Without, however, for a moment questioning his having really intended his paper to be understood as he explains (in fact, few actions can be more discourteous or unjust than to question anyone's explanation of his own words), I must say that it does not seem to me that he succeeded in clearly indicating his meaning in his former paper. He not only did not dwell upon, but he did not even mention the physical processes which would result from the actual, as distinguished from the optical, overlapping of the gaseous envelopes surrounding the component stars of the clusters he considered. It was a natural mistake, therefore, to suppose that his remarks referred only to optical over-lapping. My remarks applied only to his reasoning so misunderstood.

Observations of Shooting Stars in 1877. By W. F. Denning, Esq.

The following table of radiant points observed in 1877 is presented as a continuation of my paper in the *Monthly Notices*, vol. xxxvii., pp. 105–115, giving a similar list of results for 1876. In 1877 the paths of 1,929 shooting stars were registered, and of this total the great majority (1,593) were seen during the last half of the year. The estimated magnitudes were as follow:—

> 1st.	= 1st.	= 2nd.	= 3rd.	= 4th.	= 5th.		Total.
45	118	346	491	636	293	=	1,929

Observations were made nearly every fine night in the absence of moonlight, and were about equally divided in the aggregate between the morning and evening hours. In the table of radiants I have given the number of meteors traced from each centre, and have affixed notes to them, which render any detailed preliminary references unnecessary. The chief radiants were:—

No.	Period.	Radiant. ° °	No. of ↓s.	No.	Period.	Radiant. ° °	No. of ↓s.
1	Jan. 4-20	295+53	20	97	Oct. 2—Nov. 13	104+49	36
49	July 6—Aug. 16	7+37	37	99	Oct. 2—Nov. 13	105+12	33
62	July 6—Aug. 16	334+44	24	106	Oct. 2—Nov. 13	113+27	21
53	July 6—Aug. 16	7+53	20	117	Oct. 2—Nov. 13	101+73	23
52	July 6—Aug. 16	315+50	21	113	Oct. 2–18	133+79	22
65	Aug. 10	43+58	285	89	Oct. 15–20	92+1	57
109	Sept. 4—Oct. 19	84+54	26	118	Oct. 15—Nov. 13	130+1	30
108	Sept. 4—Oct. 19	108+38	20	122	Oct. 28—Nov. 13	43+2	31

of the satellites as seen with large or small telescopes. But I find nothing in Mr. Stone's own words which can be so interpreted.

In getting the radiants I have been careful to consider the lengths of the apparent paths and specialities of motion and appearance in the various meteors. When this is carefully done there is little danger of *pseudo*-radiants, even in cases of feeble showers. A proportion of new radiants will be found amongst my observations.

Generally I find meteors far more abundant during the last half of the year than during the first half, but my notes chiefly have reference to the former period, so that I cannot give a fair comparison. For the six months, July–December, the following are the periods of watching and number of meteors seen (omitting the *Perseïds*) during the last two years:—

Month 1876–77.	Obs. A.M. Hours.	Meteors.	Obs. P.M. Hours.	Meteors.	Obs. A.M. & P.M. Hours.	Meteors.
July	13	133	24¼	166	37¼	299
August	17¼	271	30	303	47¼	574
September	12	146	15¼	118	27¼	264
October	24¾	366	47	362	71¾	728
November	24¼	290	34¼	278	58½	568
December	11	139	24¾	157	35¾	296
July—Dec.	= 102¼	1345	175½	1384	277¾	2729

The table shows the remarkable frequency of shooting stars in the morning as compared with the evening apparitions. In any calculation of the horary numbers from these figures, one-sixth of the periods of watching should be deducted as allowance for time spent in registering the paths.

No. 1877.	Date. 1877.	Radiant Point. R.A. °	Dec. N. °	No. of Meteors.	Notes and Comparisons.
1	January 4–20	295	+53	20	Strong shower; maximum Jan. 17; meteors bright, rather slow.
2	January 4–20	57	−12	7	Very fine, slow meteors; maximum Jan 4–8; radiant near γ *Eridani*.
3	January 4–20	220	+52	5	Meteors rather slow; also a radiant at 231° + 54°, Jan. 4 (*Quadrantids*).
4	January 4–20	209	+67	7	Rapid white meteors; near α *Draconis*.
5	January 14–17	260	+45	6	Short, small meteors; a morning shower.
6	January 4–20	248	+73	7	Very slow meteors; new shower near γ *Ursæ Minoris*.
7	January 4–20	84	+74	6	Uncertain. Short, quick meteors. 110° + 70°, Jan. 27, 1868 (Zezioli's obs).
8	January 4–20	130	+44	11	Strong shower; maximum Jan. 15 A.M. Bright meteors.
9	January 4–20	250	+25	10	A feeble morning shower in *Hercules*. Comet of 1672, 256° + 20°, Jan. 20.
10	January 4–20	209	+15	4	Probably two separate showers A.M.; near α *Boötis*. Slightly seen in Jan. 1878 also.
	January 14–20	211	+ 4	5	Swift meteors, with streaks. Tupman, 200° + 2°, Jan. 8–11, 1870.
11	January 14–20	179	+47	9	Uncertain. Confirms S. & Z. 175° + 48°, Jan. 6.
12	January 4–20	160	+28	6	Very swift, white meteors.
13	February 15–20	236	+11	10	Exact. Strong shower A.M., near α *Serpentis*.
14	February 20	263	+36	6	Exact. A pair of rich A.M. showers; rapid bright meteors.
15	February 20	181	+34	8	Exact. Corder, 263° + 36°, January 27, 1877.
16	February 15–20	261	+ 4	3	Feeble; uncertain. Meteors with streaks; radiant near β *Ophiuchi*.
17	February 15–21	210	−13	5	Uncertain. Meteors very swift. Tupman, 202° − 9°, February 13.
18	February 4–20	167	+ 5	5	Meteors slow. Tupman, 165° + 4°, January 8–10.
19	February 2–4	105	+29	4	Meteors slow; rather uncertain. Near α *Geminorum*.

No.	Date.	Radiant Point. R.A.	Dec. N.	No. of Meteors.	Notes and Comparisons.
1877.	1877.	°	°		
20	February	234	+32	12	Well defined and active shower. Confirmed by other observations.
21	February	120	−5	9	A new shower in *Monoceros*.
22	March 14	263	+48	6	Exact. Confirms S. & Z. 260° + 46°, March 31.
23	March 14	218	+36	6	Active shower; meteors slow; near *β Boötis*.
24	March 14–19	320	+44	4	Morning showers; near *α Cygni*.
	April 16–19	315	+38	10	Confirms Greg (49), 307° + 40°, March 15—April 21.
25	May 15–17	299	+36	4	Confirms S. & Z. 301° + 37°, May 22–24. ? same as preceding.
26	March 8	189	+26	6	Active shower. Confirms S. & Z. 187° + 24°, April 11–19.
27	March	175	+10	5	A very good shower of bright meteors.
28	March 17	252	+12	3	Exact. One meteor nearly stationary.
29	March 17	330	+33	?	Strongly suspected. Corder, 325° + 34°, March.
30	April 16–19	303	+13	8	= Comet V. 1857, 302° + 11°, April 4. Confirmed by other observations.
31	May 15	304	+7	4	Slightly seen. Distinct to the preceding?
32	March 14	280	−14	2	Meteors very swift; one long, continued shower.
	April 16–19	289	−15	4	Confirms Greg (52), 290° − 10°, March 25—April 30.
33	March 1–15	300	+80	9	Bright, slow meteors; exact.
34	April 16–19	286	+5	12	Rather uncertain. = Comet II. 1844, 288°·5 + 5°, April 21.
35	March 14	277	+25	5	Exact. Well seen, A.M.; near *β Lyræ*.
36	April 16–19	267	+21	6	Confirms Greg, April 20, 267° + 25°.
37	April 16–19	298	+25	6	A new A.M. shower in *Vulpecula et Anser*.

38	April 16–19	260 + 2	4	Confirms Tupman, 256° − 2°, April 27.
39	April 16–19	228 − 2	5	Exact. A rich shower. Confirms G. & H. (53), 227° − 5°, Mar. 20–May 29.
40	April 16–19	269 + 37	7	*Lyraids.* At 274° + 37° in 1873 and 1874. = Comet I. 1861.
41	March 14	244 + 50	6	These showers are possibly connected together. Mean position at 250° + 51°. S. & Z. 246° + 46°, April.
	March 14	250 + 54	4	
	April 16–19	255 + 50	5	
42	April 16–19	218 + 25	4	Uncertain. Probably the same shower. Seen at 220° + 21° on April 21, 1874. (Greg 53*a*.)
	May 7	219 + 17	3	
43	May 15–17	354 + 41	4	Heis, 355° + 42°, May 20–29. An A.M. shower.
44	May 17	283 + 55	4	*Draconids.* Short, rapid meteors. Seen also in 1873 at 283° + 59°, April 21.
45	April—May	258 − 12	11	*Serpentids.* Active shower. Greg (80–85), 264° − 11°, June, July, and Aug.
46	May	210 − 10	3	A rich and probably new shower.
	April—May	203 − 8	?	Seen also by Corder at 208° − 6°, April—May 1877.
47	June 15–17	302 + 64	5	Not very certain.
48	June 15–17	285 + 32	4	Confirms Tupman, 280° + 29°, June 29, 30.
49	July 6–17	4 + 35	21	A very active shower; radiant not very precise. *Andromedes* I.
	August 3–16	10 + 38	16	Maximum about July 7–11. Greg (103), 7° + 33°, July 21—September 3. = Comet II. 1870, 3°·5 + 38°·5, August 14–.
50	July 6–17	0 + 26	4	Distinct to preceding. Radiant near α *Andromedæ.*
	August 3–16	4 + 20	7	Denza, 2° + 29°, August 8–13.
51	July 6–17	271 + 8	5	Uncertain. Meteors very slow.
52	July 6–17	315 + 48	8	Active shower; radiant near α *Cygni.* *Cygnids* I.
	August 3–16	315 + 51	13	Swift, white meteors. Greg (81), 309° + 48°, July—August.

No. 1877.	Date. 1877.	Radiant Point. R.A. °	Dec. N. °	No. of Meteors.	Notes and Comparisons.
53	July 6–17	6	+53	11	Exact. A fine shower of *Cassiopeids*; maximum about July 12–17. Streaks.
	August 3–16	8	+53	9	Swift, white meteors with streaks. S. & Z., 7° + 50°, July 11—Aug. 7.
54	July 6—August 16	350	+37	11	Active shower; possibly new.
55	July 6–17	290	+43	8	Well-marked radiant near δ *Cygni*.
	August 3 16	292	+48	7	Confirms G. and H., 293° + 42°, August 10, 1871.
56	July 6—August 16	349	+53	14	Observed chiefly in July. Short, quick meteors.
57	July 6–20	317	−11	13	Strong maximum on July 20. Tupman, 313° − 6°, July 21, 22.
58	July 6–17	336	− 7	5	The *Aquarids*. Greg (109), 337° − 6°, July 5—October 31!
59	August 3–16	342	−12	10	A distinct shower to the preceding; near α *Aquarii*.
60	July 6—August 16	333	+26	11	Chiefly observed in July. Greg (96), 338° + 26°, July—August 15.
61	July 6–17	338	+10	10	Confirms Greg (97), 338° + 17°, June 28—September 15.
	August 3–16	343	+12	8	The same shower, long continued; near α *Pegasi*.
62	July 6–17	334	+43	6	The *Lacertids*. The real centre for this shower is at 334° + 48°.
	August 3–16	335	+45	18	Very active in August; meteors very swift.
63	July 6–17	47	+45	5	Apparently a very long-continued shower, near α *Persei*, and quite distinct to the *Perseids* of August 10 near η.
	August 3–16	46	+45	6	
	September 4–16	47	+45	7	= Comet of 1764, 49° + 45°·5, July 25.
64	July 6–17	36	+47	6	A closely bordering shower to the last at θ *Persei*. = Comet of 770, 39° + 45°, July 8.
65	August 3–7	40	+56	20	*Perseids*. Meteors very swift, invariably with streaks. The four positions are exact, and probably show a distinct advancement of the radiant in R.A. Horary number from 43° + 58° on 10th, 57.
	August 10	43	+58	285	
	August 12	50	+55	14	
	August 16	60	+59	5	

66	July 6–17	280+57	5	*Draconids.* Seen by many other observers in July and August.
67	July 6–17	285−12	5	Well-marked shower; meteors very slow and bright.
	August 3–16	283− 9	3	Uncertain and feeble in August. Greg (204), 285°−13°, June 24—July 6.
68	July 6–17	298− 8	5	Uncertain. Meteors slow. Schmidt, 294°−7°, June, July, August.
69	July 6–17	258+37	6	Exact. Seen by Prof. Herschel, July 16, 1871, at 257°+36°.
70	July 6–17	313+33	5	Not exact. Meteors very slow. *Cygnids* II. Greg (101), 315°+30°, July–Aug.
71	July 6—August 16	330+70	13	Short, swift meteors; near β *Cephei*; seen chiefly on August 12.
72	July 6–17	319+ 9	?	Rather uncertain. Feeble. Greg (118), 318°+7°, July 5—August 31.
73	August 3–16	40+28	8	Meteors rapid. Tupman and Schmidt, 39°+28°, August 22–28.
74	September 16	50+31	4	Meteors slow. Heis, 48°+35°, Sept.—Oct. 15. Distinct to preceding.
75	August 3–16	22+46	7	Maximum August 4. A new shower at γ *Andromedæ*.
	September 4–16	28+45	4	Doubtful in September. 22°+45°, September 5–12, Zezioli's observations.
76	August 3–16	330+10	12	Active shower; radiation somewhat diffuse.
77	August 10–12	70+65	10	A well-marked new shower in *Camelopardalis*.
78	August 3–16	18+63	15	*Cassiopeids.* Meteors swift and slow. ? two showers here.
79	September 4–16	16+54	8	These showers are probably distinct to the preceding.
	October 2–19	21+55	5	Confirms Greg (173), 14°+57½°, Sept. 15–Nov. 15. Heis, 21°+55°, Oct.
80	August 3–16	306+6	4	*Aquilids.* Slow meteors. Feeble shower in 1877.
81	July 28—August 7	147+62	3	Not exact. Fine, slow meteors. Schmidt, 148°+67°, August 8–17.
82	August 12–16	31+18	11	Strong maximum on August 12; meteors with streaks; a new A.M. shower.
83	August 3–16	61+48	11	A well-marked shower; exact; maximum August 12–16; suspected also in September (5 meteors).
84	August 3–16	287+45	3	Feeble and uncertain. G. & H., 285°+44°, August 2—September 25.

No.	Date.	Radiant Point. R.A.	Dec. N.	No. of Meteors.	Notes and Comparisons.
1877.	1877.	°	°		
85	August 3–16	307	+67	5	Well seen by Corder on August 17, 307° + 65° (11 meteors).
	September 4–16	314	+66	5	Previous shower continued; not certain. Heis, 311° + 65°, Sept. 16–30.
86	July 6–17	295	+85	6	A series of showers not far from *Polaris*. In July–August, at 307° + 82°.
	August 3–16	320	+80	5	= Comet III. 1853. 300° + 80°, August 13.
	September 4–16	330	+78	4	Feeble and uncertain in September.
	October 2–20	310	+77	17	Active shower, with maximum on October 3–4. Meteors rather slow.
87	September 12	10	+ 0	3	Not very certain. Seen only on September 12.
88	September 4–16	242	+46	8	Radiant diffuse and inexact.
89	September 15, 16	88	+17	8	Beginning of the *Orionids*? A new A.M. shower.
	October 15–20	92	+15	57	*Orionids*. Maximum October 19, A.M. Very swift meteors; 47 had streaks.
90	September 15, 16	156	+41	5	Exact. Meteors swift, with streaks; just before daybreak.
	October 2–18	153	+42	4	Exact. A new radiant close to μ *Ursæ Majoris*.
91	August 3–16	30	+36	13	Exact. Active shower at β *Trianguli* of short, quick meteors.
92	September 4–16	160	+78	6	Not very exact. Swift, white meteors.
	October 2–18	167	+75	5	Meteors with streaks; a feeble shower.
93	September 4–16	267	+20	4	Not certain. Meteors very slow. Schmidt, 264° + 25°, July 5–31.
94	September 4–16	220	+78	10	A well-defined and active shower in *Ursa Minor*.
	October 2–18	213	+79	8	Meteors swift; less well defined in October than in September.
95	September 7–16	87	+34	10	Very swift meteors, with streaks. Maximum September 16. At θ *Aurigæ*.
96	September 4–16	61	+36	15	Exact. A rich shower; meteors very rapid, with streaks. Max. Sept. 7–16.

97	September 4–16	100 + 58	10	Probably one long-enduring radiant, at 103° + 52°.
	October 2–19	105 + 50	15	Very exact in October. Meteors short and very rapid.
	October 28—November 13	103 + 48	21	An active shower; possibly new (105° + 50° and 98° + 45°).
98	September 4–16	255 + 65	6	Slow meteors; radiant fairly well defined, near *ζ Draconis*.
	October 2–5	253 + 64	4	
99	September 15–16	101 + 11	5	Slightly seen, and not very certain. Perhaps connected with following.
	October 2–19	103 + 12	22	Very rich shower; maximum October 8; meteors very rapid.
	October 28—November 13	107 + 11	11	Confirms Tupman, 107° + 12°, October 8, 1869.
100	September 4–16	284 + 62	4	Uncertain. Heis, 286° + 57°, September 16–30.
101	September 4–16	53 + 64	6	Chiefly September 5–7. Heis, 57° + 61°, October 1–15.
102	September 4–16	73 + 14	7	Meteors rapid; maximum Sept. 15. Tupman, 75° + 15°, Sept. 22, 1871.
103	September 4	40 + 72	6	An active transient shower. G. & H., 44° + 73°, September.
104	September 4–16	59 + 9	5	Meteors rapid, with streaks. Slightly seen and uncertain.
	October 28–November 13	62 + 9	6	Meteors rather slow; streaks. Schmidt, 62° + 6°, October 10–22.
105	September 7–16	49 + 14	6	Meteors rapid, with streaks. Schmidt, 51° + 14°, September 3–30.
	October 2–19	47 + 16	5	Confirmed by Corder at 47° + 14°, October 1877.
106	September 4–16	113 + 24	4	Slightly seen and rather uncertain. Beginning of *Gemellids*.
	October 2–19	115 + 29	13	Exact. Meteors rapid, with streaks. S. & Z., 113° + 29°, Oct. 21–25, 1868 (56 meteors).
	October 28–November 13	110 + 25	8	Exact. Meteors rapid; maximum on November 7. *Gemellids*.
107	September 4–16	70 + 85	7	Meteors rapid; radiant near *Polaris*.
	October 2–18	50 + 85	9	Confirms Greg $\binom{128}{143}$, 105° + 86°, September–October.

No.	Date.	Radiant Point. R.A.	Dec. N.	No. of Meteors.	Notes and Comparisons.
1877.	1877.	°	°		
108	October 2–19	108	+38	16	Meteors rapid, with streaks; suspected also in September.
109	September 4–16	85	+53	8	Maximum September 5. *Aurigids.* Radiant not very exact.
	October 2–19	84	+55	18	Maximum October 5–8; two showers in October? suspected also in November.
110	September 15	130	+46	4	Meteors rapid, with streaks; seen just before daybreak.
	October 2–19	130	+47	11	Active shower; maximum October 15, 16; seen by several other observers.
111	September 16–October 7	20	+ 8	8	Not well seen; maximum September 16; meteors very slow.
112	September 4–16	72	+41	6	Meteors rapid. Confirms Corder, 75°+45°, October (14 meteors).
113	October 2–18	133	+79	22	Exact. Extremely well marked and certain shower, with maximum about October 3–4. Meteors very rapid, with streaks. = Comet I. 1825, 133°+77°, October 7.
114	October 2–18	138	+58	10	New shower of rapid meteors in *Ursa.*
115	October 2	225	+52	7	Exact. Fine shower of slow, bright meteors; new and rich radiant.
116	October 16, 17	178	+34	4	Exact. Short, quick meteors; new. At dawn.
117	October 2–19	100	+73	9	Long-continued and rich shower of very swift meteors in *Camelopardalis.*
	October 28—November 13	102	+73	14	Exact. Strong max. on Nov. 8, A.M. Seen by S. & Z. and Gruber on Oct. 28.
118	October 15–19	133	+21	18	Exact. Maximum Oct. 14–15. A shower of very, very swift meteors.
	October 28—November 13	127	+17	12	Exact. Maximum November 12–13. Long paths, not often with streaks.
119	October 15–18	120	+15	9	Exact. Distinct to preceding; meteors very rapid.
120	October 15–18	140	+28	9	Exact. Meteors with streaks; one meteor stationary. Near ϵ *Leonis.*
121	October 2–18	186	+71	6	Exact. Very feeble shower; meteors small, rather slow. Compare No. 157.
122	October 8	47	+28	8	Short, quick meteors. Seen only on October 8.
	October 28—November 13	43	+22	31	Exact. A very rich shower of rather slow meteors; max. Oct. 28—Nov. 1.
123	October 2–18	61	+47	8	Maximum October 8. Bright meteors; very rapid, without streaks.
	October 28—November 13	61	+49	8	Maximum November 4. Continuation of preceding.

124	October 2–18	31 + 39	10	Active shower. Strong maximum on October 8. Short meteors, not very swift.
125	October 2—November 13	8 + 35	7	Rapid. Chiefly on October 8. Meteors swift.
126	October 2–18	77 + 31	9	Exact. Max. Oct. 8. Meteors with streaks. Tupman, 77° + 30°, Oct. 13.
	October 28—November 13	77 + 32	8	Exact. Maximum November 7. Meteors without streaks.
127	October 2–19	133 + 68	8	Meteors rapid. Possibly a new shower.
128	September—October 18	42 + 59	9	*Perseïds.* Not very certain. Confirms Greg (168), 46° + 55°, November.
	October 28—November 13	45 + 60	6	
129	October 2–5	290 + 70	7	Exact. Meteors rather slow. Confirms Heis.
130	October 5–8	56 + 52	4	Rather slow meteors. = Comet VI. 1847, 54° + 52°·5, October 4?
131	October 15–19	121 ± 0	6	Meteors very swift; A.M. showers. Tupman, 124° + 4°, November 7.
132	October 15–19	100 + 1	5	Tupman, 101° ± 0°, October 16.
133	October 17–19	63 + 22	7	The *Taurids* I. Meteors rather slow. A somewhat feeble shower in 1877. Average radiant position 60° + 20°.
	October 28—November 13	57 + 18	9	
134	October 8—November 13	32 + 50	8	Meteors slow. Seen chiefly on October 8. New radiant at *θ Persei.*
135	October 15–19	86 + 8	7	A subradiant of the *Orionids.* Tupman, 86° + 6°, October 8.
136	October 2—November 13	80 + 21	9	Meteors very swift. Radiant rather uncertain near *ζ Tauri.* Compare No. 152.
137	October 2—November 13	4 + 50	7	Inexact. Confirms G. & H., 5° + 55°, Oct., and Schmidt, 5° + 53°, Oct. 22–28. A radiant also strongly suspected at 11° + 70°, October 8–14.
138	October 28—November 13	30 + 16	11	Not very swift meteors. Greg (154), 34° + 23°, October 19—November 20.
139	October 28—November 13	133 + 31	10	Very swift meteors with streaks. Maximum November 10–12.
	November 25—December 13	131 + 32	6	Suspected also in September. Maximum December 2.
140	October 28—November 13	60 + 34	12	Meteors rather slow. Suspected at 60° + 37°, November 25—December 13.
141	October 28—November 13	19 + 30	7	Meteors rather slow. Radiant fairly exact. Gruber, 20° + 26°, Oct. 17–22.
142	October 28—November 13	25 + 46	4	Confirms Heis, 24° + 42°, November 1–15. Perhaps distinct to *Andromedes.*
	November 25–27	24 + 45	9	*Andromedes.* Meteors very slow. Maximum Nov. 25; very feeble on Nov. 27.

No.	Date.	Radiant Point. R.A.	Dec. N.	No. of Meteors.	Notes and Comparisons.
1877.	1877.	°	°		
143	October 28—November 13	40	+77	9	Exact. New shower of rather slow meteors. Maximum October 31—November 4. Suspected in December also.
144	October 28—November 13	338	+59	6	Maximum Nov. 10. Confirms Heis, 338° + 56°, October 17—November 15.
145	October 28—November 13	97	+25	8	Rapid meteors. Maximum November 4. = *Gemellids* No. 106?
146	October 28—December 13	173	+11	5	New A.M. shower near β *Leonis*. Meteors very rapid.
147	October 28—November 13	40	+10	5	Maximum November 9. Confirms Tupman, 43° + 11°, October 13.
148	November 12	125	+40	5	Exact. One meteor stationary.
149	November 10–13	148	+24	5	A feeble return of the *Leonids*. Meteors very rapid, with bright white streaks.
150	October 28—November 13	70	+65	6	Exact. Maximum November 13, A.M. Well seen in 1876 at 69° + 66°.
151	December 1–13	109	+33	9	*Geminids*. Rapid, short meteors.
152	November 25–December 13	80	+25	6	*Taurids* II. Meteors very slow. Well seen in 1876. Sawyer, 80° + 22°, Dec. '77.
153	December 8–12	145	+ 7	8	Exact. Strong maximum Dec. 8, A.M. Meteors very swift, with streaks.
154	December 10–13	199	+19	5	Short, swift meteors. Just before daybreak. Slightly seen.
155	December 10–13	221	+43	3	A morning shower at β *Hercules*. S. & Z., 220° + 40°, January 19.
	January 4–9	221	+39	6	Meteors rapid, with streaks.
156	December 10–13	230	+33	3	Short, quick meteors. Just before daybreak. Slightly seen.
157	November 25–December 13	185	+71	9	A strong shower; maximum November 29. Rapid meteors with streaks.
158	November 25–December 13	70	+15	5	Rather uncertain. Meteors not very swift.
159	November 27	54	+48	4	Meteors swift; fairly exact. Seen while watching for *Andromedes*. Probably a rich shower. Weiss, 52° + 49°, Novovember 27, 1872.
160	November 27–December 13	87	+37	5	Diffuse and rather uncertain. Heis, 95° + 33°, December 1–15.
161	November 25–December 13	102	+45	4	Uncertain. Different to *Geminids*.
162	December 10–12	166	± 0	3	Meteors very rapid, with streaks. Only slightly seen.

Radiant Points deduced from the Paths of 4,143 *Shooting Stars observed by the Members of the Italian Meteoric Association in the year* 1872. By W. F. Denning, Esq.

The radiant points given in the table which follows were determined by the projection of the meteors in the Catalogue entitled *Osservazioni di stelle cadenti fatte dai membri dell' Associazione Meteorica Italiana durante l'anno* 1872. (Milan 1874). The book contains 7,512 paths in all, numbered progressively; but of these no less than 3,621 (Nos. 2684 to 6304) are for August 4–13, and chiefly *Perseïds*, which it was not considered essential to examine thoroughly. The number of meteors projected was 4,143, and the radiant points deduced therefrom 315, of which, however, a large number are duplicates of the same showers. Less than 200 distinct systems are probably shown. The meteors examined and resulting radiants for each month were as follow:—

No. in Table.	Period of Observations.	Length in days.	Meteors Projected.	Numbers in Catalogue.	Radiants Reduced.
I.	Jan. 1–15	15	313	1 to 313	30
II.	Feb. 1—Mar. 12	42	381	344 „ 724	24
III.	Mar. 31—April 12	13	542	731 „ 1272	37
IV.	May 3–15	13	269	1319 „ 1587	22
V.	May 26—June 13	19	356	1588 „ 1943	31
VI.	June 26—July 11	16	459	1961 „ 2419	36
VII.	July 15—Aug. 2	18	264	2420 „ 2683	27
VIII.	Aug. 6–12 (A.M.)	7	413	2900 „ 6274	25
IX.	Aug. 24—Sept. 14	22	455	6307 „ 6761	36
X.	Oct. 29—Nov. 13	16	255	6820 „ 7074	24
XI.	Nov. 25 to Dec. 31	37	436	7077 „ 7512	23

Thus the observations (and reductions) are broken up into periods of from about 13 to 22 days. The intervening voids were caused by moonlight. In two cases (Feb.–March and Nov.–Dec.) I incorporated two following sets of observations, because the radiants were generally the same, and the meteors observed, if taken singly, insufficient to indicate many of the positions accurately. Only 413 of the large number between August 4–13 were projected. These were seen in the mornings, and nearly all of them selected as probably belonging to radiants eastwards of the *Perseïds*, and perhaps to new showers or to showers feebly observed before. There are no reductions for October, only 37 meteors having been observed during that month. The tracks were pencilled upon the star charts prepared by the Luminous Meteor Committee of the British Association for the use of their observers. Some difficulty was occasionally

found in getting at the radiant points, for the shooting stars recorded during several of the periods presented singular discordances in their directions, and were a very confused mass, requiring much care in assigning the true centres. In such cases it is obviously very important to consider the length of path and observed velocity of the individual meteors. The difficulty mentioned in sorting them arose not so much from errors of observation as from the large number of showers in contemporaneous operation, and the closely bordering positions of some of them.

The majority of the radiants in the table agree with showers previously discovered and included in the catalogues of Greg and Herschel, Heis, Schmidt, Schiaparelli and Zezioli, Tupman, and others, and accord in many instances with the resulting radiants from my own observations of shooting stars during the last two years. A proportion of the showers are new, and a few of them apparently rich systems (agreeing in several cases with cometary dates and radiants), which it will be desirable to re-observe and confirm in future years.

The chief radiant points observed were the *Perseïds* (August 1–13) and *Andromedes* (November 27). A number of minor systems were also fairly active; and to what extent may be found by a reference to the table, in which I have given the number of meteors conforming to each radiant; but the figures relating to this detail should not be considered exact. In cases where a meteor converged on two or more radiants in the same line it may have been counted twice over, and when far away from the radiant one will sometimes have been omitted altogether. Precision in this matter is not very important.

Many of the showers endured apparently for much longer periods than is usually attributed to them. Thus the *Perseïds* (or a succession of coincident radiants in *Perseus*) continued in feeble action during the whole of the last five months of the year, and the *Coronaids* were seen between March and July. Many additional samples of long duration will be found on inspecting the table, and my own observations are strongly corroborative in this particular. It appears certain that shooting stars continue to fall, and sometimes abundantly, from radiant points long after the first and maximum display, which usually occurs when the radiant is near the apex of the Earth's way.

In going over this Italian catalogue a few observed peculiarities struck me which may be worthy of note here. On May 3 Signor Maggi, at Volpeglino (who appears to have been one of the most successful of the observers), saw three meteors in succession with identical paths. They were of the 2nd mag., and very swift, the observed tracks being from 238°+78° to 16°+88°, and belonging to the radiant No. 11, IV., in the table, from which others were seen on the same night. The same observer recorded on Aug. 10, $11^h\ 19^m$ to $15^h\ 19^m$, five shooting stars also with exactly similar paths, namely, from 91°+59° to 101°+59° (*see* Nos. 4971, 4981, 5062, 5080, 5146).

Another was seen on the same night from 91°+59° to 98°+60° (No. 5102). These foreshortened paths conform to the radiant in *Auriga*, No. 10, VIII. Nos. 4997, 4998, and 5007 supply another instance of similar paths (15°+35° to 1°+14°, *β Andromedæ* to *γ Pegasi*), and belonged to the *Perseïds*. Not many stationary meteors were registered during the year. One at 76°+46° (No. 5017 1st mag.), on August 10, strongly supports the shower No. 11, VIII. Another at 33°+33° (No. 3001), on August 5, agrees with No. 19, VIII. A fireball, comparable to the Moon, was seen stationary by Maggi on August 25, at 10°+7° (No. 6319). He also noted a meteor at 48°+49° (No. 5036), without movement, on August 10. The latter observation is valuable as confirmation of the radiant No. 14, VIII., in *Perseus*, which is a well-marked shower in August, and quite distinct to the ordinary *Perseïds*, at about 44°+56°. Other stationary meteors seen registered as follows: at 146°+9°, on April 3 (No. 926); at 163°+58°, on May 9 (No. 1512); at 191°+47°, on May 15 (No. 1586); and at 215°+16°, on July 3 (No. 2186).

The length of the meteor paths, as given in this catalogue, is, I believe, greater as a rule than the average in the lists of Zezioli, Heis, or Tupman, a large number of which I have recently been engaged in examining.

So far as can be judged by comparing the resulting radiants with those obtained by previous observers, and more lately by myself, the meteors in this Italian catalogue appear to have been very carefully and accurately registered.

Ashleydown, Bristol,
January 1878.

Radiant Points of Shooting Stars, deduced from Observations by the Members of the Italian Meteoric Association in the year 1872. By W. F. Denning.

No. of Radiant.	I. Jan. 1–15. 313 Meteors. Radiant. R.A. °	Dec. °	No. of ←s.	II. Feb. 1–March 12. 381 Meteors. Radiant. R.A. °	Dec. °	No. of ←s.	III. March 31–April 12. 542 Meteors. Radiant. R.A. °	Dec. °	No. of ←s.	IV. May 3–15. 269 Meteors. Radiant. R.A. °	Dec. °	No. of ←s.	V. May 26–June 13. 356 Meteors. Radiant. R.A. °	Dec. °	No. of ←s.	VI. June 26–July 11. 459 Meteors. Radiant. R.A. °	Dec. °	No. of ←s.	VII. July 15–August 2. 264 Meteors. Radiant. R.A. °	Dec. °	No. of ←s.	VIII. August 6–12 A.M. 413 Meteors. Radiant. R.A. °	Dec. °	No. of ←s.	IX. August 24–Sept. 14. 455 Meteors. Radiant. R.A. °	Dec. °	No. of ←s.	X. Oct. 29–Nov. 13. 255 Meteors. Radiant. R.A. °	Dec. °	No. of ←s.	XI. Nov. 25–Dec. 31. 436 Meteors. Radiant. R.A. °	Dec. °	No. of ←s.	No. of Radiant.
1	184	+52	6	182	+56	20	209	+ 6	25	202	+ 7	14	201	+11	9	53	+62	7	47	+57	6	44	+56	—	45	+57	12	47	+56	4	46	+57	7	1
2	109	+34	8	105	+30	12	201	+18	15	202	+17	17	293	+14	22	290	+15	20	291	+17	5	100	+71	38	339	+46	24	100	+71	5	132	+76	8	2
3	178	+35	9	180	+36	12	184	+35	15	186	+36	8	288	+55	15	279	+59	26	284	+60	9	283	+63	6	280	+60	9	280	+70	5	285	+79	5	3
4	80	+23	11	83	+24	15	228	− 5	9	229	− 4	18	273	− 3	11	273	− 2	20	272	+ 4	7	61	+39	35	67	+35	10	62	+36	10	62	+38	9	4
5	164	+35	5	158	+28	21	162	+34	13	162	+34	10	312	+22	16	313	+20	7	320	+23	11	70	+64	31	64	+65	13	73	+65	9	68	+65	7	5
6	49	+61	7	53	+66	13	204	− 8	7	209	− 8	9	343	+16	8	344	+12	9	337	+14	10	50	+75	28	40	+73	10	45	+70	6	40	+70	8	6
7	130	+24	7	129	+22	19	139	+16	8	252	+47	8	250	+45	5	250	+48	10	349	− 1	6	50	+85	4	68	+84	15	54	+ 6	10	79	+57	7	7
8	155	+64	10	145	+55	5	157	+45	12	150	+45	7	257	+30	14	255	+37	12	256	+37	11	92	+56	18	100	+56	10	100	+60	6	265	+65	4	8
9	104	+53	13	133	+67	10	133	+66	20	135	+68	10	233	+60	15	250	+62	18	233	+68	7	76	+33	21	78	+23	8	78	+37	7	245	+65	4	9
10	228	+53	8	260	+66	6	258	+60	20	315	+55	6	312	+63	5	310	+51	13	318	+49	9	81	+57	17	315	+47	9	189	+57	8	180	+64	3	10
11	221	+43	7	111	+62	22	232	+54	17	233	+47	24	305	−10	8	303	+ 7	7	302	+ 7	4	75	+45	13	343	+ 9	11	147	+ 2	6	346	+47	7	11
12	241	+63	6	28	+35	16	170	+78	7	180	+79	6	285	± 0	8	15	+35	11	13	+29	12	4	+31	4	10	+34	20	9	+37	6	102	+48	5	12
13	100	+42	6	101	+44	12	106	+52	10	175	+26	6	353	+39	7	8	+54	6	8	+54	15	52	+20	11	302	+ 9	9	62	+20	13	64	+15	7	13
14	104	+14	7	177	+10	32	179	+ 9	15	170	−10	9	35	+47	7	163	+59	9	167	+55	5	49	+47	11	44	+43	7	157	+17	8	26	+46	202	14
15	204	+45	8	205	+51	7	206	+44	30	210	+48	18	215	+55	17	216	+47	10	351	+53	11	45	+24	8	45	+20	8	45	+26	7	48	+32	7	15

16	140+77	8	147+ 4	17	147+ 1	7	203+39	9	10+80	6	64+46	10	65+51	12	60+48	22	63+48	12	62+47	10	105+14	6	16
17	40+77	8	300+80	12	215+83	10	300+79	7	279+77	10	290+82	8	315+60	10	87+34	8	180+75	10	135+21	12	330+66	6	17
18	65+65	10	77+60	9	235+25	19	241+26	9	246+21	9	248+22	16	30+47	10	34+50	3	286+45	5	160+40	8	170+84	5	18
19	140+30	6	210+70	14	210+66	22	218+62	7	215+70	7	210+62	7	23+37	7	33+33	7	34+35	14	130+45	8	110+35	10	19
20	150+43	7	10+60	8	23+62	5	247+57	8	5+17	5	335+26	10	337+28	9	347+36	4	140+76	5	180+32	6	24+27	4	20
21	245+72	7	106± 0	5	265+38	31	265+38	15	278−35	5	291+45	8	294+40	10	47+32	9	310+67	12	157+30	4	120+40	5	21
22	258+45	5	55+19	8	229+35	6	251+ 8	10	260+ 8	9	261+11	14	294− 7	6	135+66	8	252+36	5	140+66	7	125+65	4	22
23	120+46	8	123+46	5	231+ 7	17			238+13	8	235+ 7	14	287+27	10	87+15	5	17+61	23	136+11	5	20+56	7	23
24	195+63	5	288+12	6	277+10	7			192+35	7	188+29	9	260+63	5	17+ 5	5	300+82	14	88+ 7	7			24
25	120+15	6			216+14	8			214+16	12	215+16	7	285+44	8	5+20	6	24+44	7					25
26	157+13	6			338+61	18			337+59	10	325+66	8	338+64	6			342+59	11					26
27	135+48	6			195− 6	6			258−14	19	269−13	12	330+42	6			358+20	11					27
28	168+ 3	5			190+20	12			248− 3	7	253− 5	7					235+65	9					28
29	200+58	7			223+23	14			286+36	15	278+35	29					329+ 9	5					29
30	110+70	8			262± 0	10			332± 0	8	325+ 3	9					15+10	10					30
31					255+27	9			315+32	22	315+33	15					270+20	7					31
32					304+12	9					24+61	10					240+25	6					32
33					151+23	7					290+70	11					333+27	12					33
34					278+46	8					320−12	6					280+30	6					34
35					287+27	5					344+46	13					260+50	5					35
36					91+58	6					27+50	7					216+49	5					36
37					183+62	7											240+46	4					37

On the Determination of the Axial Position of Mars with respect to the Earth at any Epoch. By Richard A. Proctor, Esq.

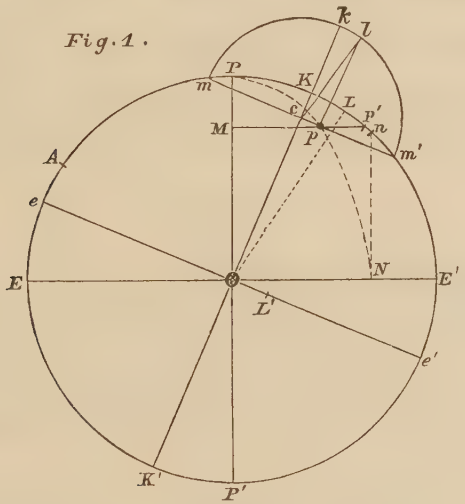
Two or three months ago I began the work of re-examining the chart of *Mars* which I formed in 1867, with the view of introducing some of the corrections suggested (1) by the observations of Messrs. Green and Knobel during the opposition of 1873, and (2) still more by the remarkably fine series of drawings made by Mr. Green during the opposition of last autumn. But hearing that many valuable observations had been made by Mr. Trouvelot with the Washington refractor, and by Schiaparelli, at Milan, and having also a wish to go systematically through some of the earlier observations, which are inadequately represented in my present chart, I found it impossible to complete the new chart so soon as I had expected. In fact, I may have to defer the communication of my results to the Society for some few months. In the meantime, however, it occurred to me that it might be well to mention what I proposed to do; in order that any observer, who had drawings of interest and might wish to see the results (if new) incorporated in the proposed chart, might have an opportunity of communicating with me, and perhaps sending tracings of such drawings. It also seemed to me that, as there are many who have drawings which they may wish to compare with the calculated actual aspect of the planet at the epochs when those drawings were made, it might be useful to indicate a comparatively simple construction by which the projection of *Mars* at any epoch may be obtained if the declination and right ascension of the planet be known at the time of observation. The last figure (fig. 4) of the present paper indicates the actual construction for two cases, the light lines representing the part of the construction common to all cases, the broken light lines representing the construction for one case, the dotted lines that for the other case, the heavy lines representing the deduced position of the axis and equator of *Mars* for the first case dealt with. It is not at all necessary in the actual employment of this construction that the explanation illustrated by the first three figures should be read through; fig. 4 and the letterpress relating to it will suffice amply for that purpose. But for the sake of completeness, and for those students of such matters who may desire to recognise the reasons on which the construction is based, and one way at any rate by which the usual formulæ for such cases can be established, I give some preliminary explanations.

In the first place let it be noted that by a simple construction we can obtain the position of the pole and equator of *Mars* with respect to the pole and equator of our heavens. Thus we have, from Oudemans' elements, which may be regarded as relating to the year 1834—

Longitude of pole of *Mars* = 349° 1′
Latitude = 61 9
} for Ecliptic, 1834·0.

Now in fig. 1 (as in the others) let POP′ represent the polar axis of the heavens, EOE′ the equator projected into a straight line. In fig. 1 (only) *e*O*e*′ is the ecliptic similarly projected, the ascending node (♈) being at O on the concavity, the descending node at O on the convexity of the sphere PP′; KOK′ the ecliptical polar axis. Thus POP′ is the projection of the equinoctial colure. Let KAK′L be an arc of 349° 1′, or KL an arc of 10° 59′; then a parallel LL′ to KO would give L′ the point on which a great circle through KK′ and the pole of *Mars* on the heavens would intersect the ecliptic. Also if K*m*, K*m*′ are arcs of 28° 51′, the straight line *mcm*′ represents the projection of a small circle having K as nearest pole, and passing

Fig. 1.



through the pole of *Mars*. The point *p* occupied by this pole might obviously be obtained by dividing *cm*′ in *p*, so that

$$cp \cdot cm' :: \mathrm{OL}' : \mathrm{O}e';$$

for then *p* would lie on an ellipse having KOK′ as major axis, and OL′ as semi-minor axis—this ellipse being the projection of the great circle through KL′K′. But the best way to obtain the point *p* is to draw the semicircle *mkm*′ on *mm*′, *cl* parallel to OL, and *lp* perpendicular to *mm*′. Or practically the best way is with a protractor to make the angle K*cl* (or *kcl*) equal to 10° 59′, and drop perpendicular *lp* to *mm*′.

Having thus obtained *p*, the northern pole of *Mars* on the heavens, we have only to draw M*pp*′ perpendicular to PP′ to obtain the arc P*p*′, which is the north polar distance (I) of the pole of

Mars. The right ascension of p is obtained by dividing OE′ in N in the same proportion that Mp′ is divided in p, and drawing Nn perpendicular to OE′; for then an ellipse having PP′ as major axis and passing through p will pass through N and be the projection of an hour-circle through p, and the inclination of this circle to the equinoctial colure POP′ is measured by the arc Pn. The right ascension of p is, however, equal to the arc PEP′n, and as the ascending node of the equator of *Mars* on the Earth's equator is 90° in advance of the pole of *Mars*, in right ascension, it follows that the arc E′n measures the right ascension, N, of the ascending node of *Mars* on the Earth's equator. This construction carefully made will give results very close to those obtained by the ordinary formulæ of spherical trigonometry, viz. :—

$$\left.\begin{array}{l}\text{R.A. of ascending node of } \mathcal{♂}\text{'s equator on } \oplus\text{'s equator (N)} = n\text{E}' = 47^\circ\ 33'{\cdot}9 \\ \text{Inclination of } \mathcal{♂}\text{'s equator to } \oplus\text{'s equator (I)} = \text{P}p' = 39^\circ\ 55'{\cdot}6\end{array}\right\} \text{ in 1834.}$$

To obtain N and I for any other date, $1834 + t$, we make the same correction precisely as for a star in same R.A. and Decl., that is, we correct for the effects of precession, getting for $1834{\cdot}0 + t$

$$\text{N} = 47^\circ\ 33'{\cdot}9 + 0'{\cdot}50\,t,$$
$$\text{I} = 39^\circ\ 55'{\cdot}6 - 0'{\cdot}23\,t,$$

with amply sufficient approximation.

In passing I may note that the spherical formulæ for obtaining N and I from λ, the longitude of ascending node of equator of *Mars* on ecliptic, and i, the inclination of equator of *Mars* to ecliptic, can be readily deduced from the constructions used in fig. 1. For regarding Mp and OM as projections of Oc and cp (and remembering that angle POK $= \omega$, the obliquity of the ecliptic) we have (putting PO equal to unity),

$$\cos \text{P}p' = \text{OM} = \sin i \cos \omega - \cos i \sin \omega \cos \lambda,$$
$$\text{M}p' \cos \text{N} = \text{M}p = \cos i \sin \omega + \sin i \cos \omega \cos \lambda,$$

that is,

$$\cos \text{I} = \sin i \cos \omega - \cos i \sin \omega \cos \lambda,$$

and

$$\sin \text{I} \cos \text{N} = \cos i \sin \omega + \sin i \cos \omega \sin \lambda,$$

the required equations.

Let us next consider the construction for obtaining the projections of *Mars* from his known declination and right ascension at any epoch, N and I being known as obtained above, or otherwise.

In fig. 2, POP′ being the polar axis of the Earth, EOE′ the projection of the celestial equator, let pOp′ be the polar axis of

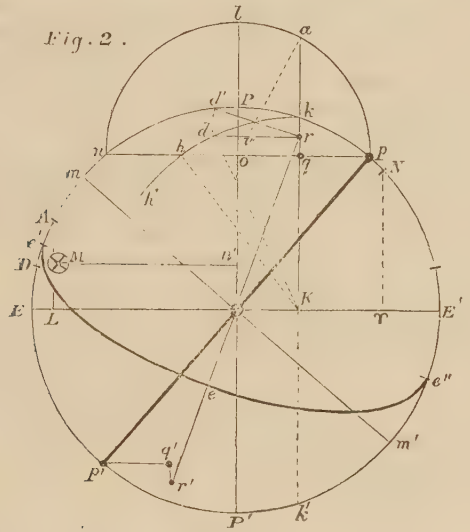
Mars, mOm' the projection of the equator of *Mars*, the ascending node on the Earth's equator being on the concavity of the sphere PP′, at O. Then, if we take arc $PN = N$, and draw $N\Upsilon$ perpendicular to OE′, Υ on the concavity of sphere PP′ is the first point of *Aries*. Now, let

R.A. of ♂ at epoch $= \alpha$,

N. Decl.„ „ $= \delta$.

Then, if arc $pPA = \alpha$, and arc $ED = \delta$, and AL, DD′ be drawn perpendicular to EO and PO respectively, *Mars* lies at M, the intersection of DOD′ with an ellipse having PO, OL as semi-axes. The polar axis and equator of *Mars's* globe would of

Fig. 2.



course be situated, in the small disk M, precisely as pOp' and mOm', the polar axis and equator of the Martian heavens, are situated in the disk PP′. And clearly, if we wish to ascertain how *Mars* at M would appear as seen from the Earth at O, we have only to suppose the sphere PEP′ so turned that M shall be brought to O; then the sphere PEP′ will be presented in precisely the same aspect as the globe of *Mars*, for the Martian celestial axis and equator, pp' and mm', are parallel to the axis and equator plane of the globe *Mars* throughout the supposed turning motion. Now, so far as the shape assumed by the equator mm' is concerned, the turning might be effected in any way we pleased so long as the centre of M was finally brought to coincidence with O. But as we wish to determine also the position of the polar axis of *Mars* with respect to the meridian, it is convenient to turn the sphere PEP′ by two motions which leave the axis

PP′ (or rather its projection) unchanged in position. Thus we first turn the sphere about the axis PP′, bringing M to D′, and carrying the Martian pole p to q (on the convexity of the sphere PP′ in this case, since MD′ lies on the concavity of that sphere); and then, secondly, we rotate the sphere about EOE′ till the centre of the globe M is brought from D′ to O, and the Martian pole from q to r. Then is rOr' the polar axis of *Mars*, Or' the remoter half, and the equator of *Mars* has the form shown by the half ellipse $e'ee''$, Oe, the minor axis, being (by a well-known property of orthogonal projection) equal to rd' drawn perpendicular to rOr'.

Now, let us consider what construction is indicated for determining the true positions of q and r.

The first rotation of the sphere, round PP′, is through an angle measured by the arc PA $= (N-\alpha)$, the right ascension of *Mars* from the ascending node of his equator on the Earth's. Thus, if we draw *pon* perpendicular to POP′, representing the projection of a declination parallel through p, and on the semicircle *pln* take arc *pa* measuring the angle $poa = (N-\alpha)$, aq perpendicular to np gives us the point q. Produce aq to k', giving kk' the projection of a small circle parallel to great circle POP′. Then we have to rotate the sphere PP′ through an angle measured by arc DE $= \delta$, the declination of *Mars* (north, positive). Hence, if khh' be part of semicircle on kk' cutting np in h, and the arc hd measures the angle hK$d = \delta$ (on side of h towards k, because obviously the rotation of D′ on concavity of sphere to O will carry q on convexity towards k), then dr perpendicular to kK gives the true position of the northern Martian pole at r; the southern will be at r', Or' on rO produced being equal to Or; q' shows the position of the southern pole at the end of the first rotation. Of course, both q' and r' are on the concavity of the sphere PP′ in the special case dealt with.

It may be noticed that

$$rd = rd';$$

for the square on rd' = rectangle under kr, rk' = square on rd. So that the construction gives Oe $(= rd')$ without occasion for drawing rd'.

Next let us consider the formulæ resulting from this construction. It will be convenient to regard the angle kKh as an auxiliary angle. Note also that

$$(hq)^2 = kq.qk' = nq.qp = (aq)^2; \text{ or, } hk = aq.$$

What we require is first the angle POr; and next the angle between the plane $e'ee''$ and the line of sight to O, or the elevation of the Earth above the plane of the equator of *Mars*. The former angle is commonly regarded as positive when r is on the east of PP′, so that in the position of r in fig. 2 this angle is negative. The Earth's elevation above the plane of the equator

of *Mars* is considered positive when the Earth is north of this plane, as in the case illustrated by fig. 2. Put then

$$\angle \mathrm{PO}r = -p$$

and

inclination of $e'e\,e''$ to line of sight to O $= l$.

Put also

auxiliary angle $k\mathrm{K}h = \mathrm{Q}$.

Then

$$\tan \mathrm{Q} = \frac{hq}{\mathrm{K}q} = \frac{aq}{\mathrm{K}q} = \frac{oa \sin aop}{\mathrm{O}o} = \frac{\sin \mathrm{I} \sin (\alpha - \mathrm{N})}{\cos \mathrm{I}},$$

$$\tan p = -\frac{\mathrm{OK}}{r\mathrm{K}} = -\frac{oq}{\mathrm{K}d \cos d\mathrm{K}k} = -\frac{aq \cot (\alpha - \mathrm{N})}{hq \operatorname{cosec} \mathrm{Q} \cos (\mathrm{Q} - \delta)} = -\frac{\cot (\alpha - \mathrm{N}) \sin \mathrm{Q}}{\cos (\mathrm{Q} - \delta)},$$

and

$$\tan l = \frac{d'r}{r\mathrm{O}} = \frac{dr}{r\mathrm{O}} = \frac{r\mathrm{K} \tan (\mathrm{Q} - \delta)}{r\mathrm{O}} = \cos p \tan (\mathrm{Q} - \delta).$$

These are the required formulæ for finding p and l.

We can also deduce from the construction the following formulæ :—

$$\begin{aligned} \cos l \sin p &= -\sin \mathrm{I} \cos (\alpha - \mathrm{N}), \\ \cos l \cos p &= \sin \mathrm{I} \sin (\alpha - \mathrm{N}) \sin \delta + \cos \mathrm{I} \cos \delta, \\ \sin l &= \sin \mathrm{I} \sin (\alpha - \mathrm{N}) \cos \delta - \cos \mathrm{I} \sin \delta. \end{aligned}$$

We note that

$$\sin \mathrm{I} \cos (\alpha - \mathrm{N}) = oa \cos aop = oq = or,$$

and

$$\sin \mathrm{I} \sin (\alpha - \mathrm{N}) = oa \cos aop = aq = hq;$$

also radius OP being throughout regarded as unity,

$$\cos l = \mathrm{O}r, \qquad \sin l = d'r = dr,$$

$$\cos l \sin p = -\mathrm{OK} = -or = -\sin \mathrm{I} \cos (\alpha - \mathrm{N}),$$

$\cos l \cos p = r\mathrm{K}$ = projection of hq and $q\mathrm{K}$ on a line through K inclined at an angle = $d\mathrm{K}h$ to $k\mathrm{K}$, towards Kh,

$$= hq \sin \delta + \mathrm{O}o \cos \delta$$

$$= \sin \mathrm{I} \sin (\alpha - \mathrm{N}) \sin \delta + \cos \mathrm{I} \cos \delta,$$

and

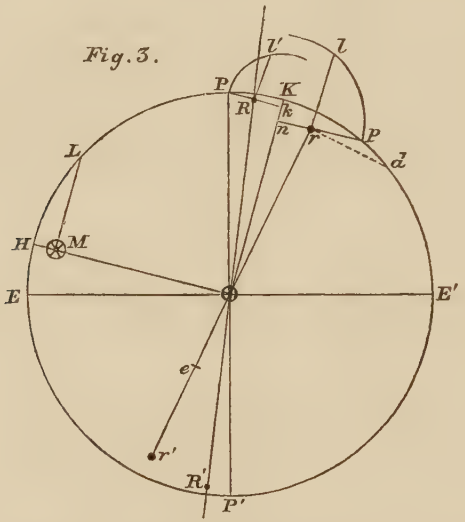
$\sin l = rd$ = projection of hq and $q\mathrm{K}$ on a line through K perpendicular to the one last mentioned,

$$= hq \cos \delta - \mathrm{O}o \sin \delta,$$

$$= \sin \mathrm{I} \sin (\alpha - \mathrm{N}) \cos \delta - \cos \mathrm{I} \sin \delta$$

In some cases, where, for instance, the student may have had occasion to determine accurately the projection of the centre of M on the disk PEP′, it may be convenient to carry M to O by a single rotation. Fig. 3 illustrates this method. We join MO; draw ML and OK perpendicular to MO, and pn, Pk to OK. Then taking arcs Pl, Pl' about centres n and k respectively, each measuring the same angle as arc KL, we draw lr and $l'R$ parallel to OK, giving rOr' the position of north Martian pole, and ROR′ the

Fig. 3.



changed position of the polar axis POP′ corresponding to the rotation round KO. The angle ROr is the same as POr from the former construction and the minor semi-axis Oe, the equator of *Mars*, is given by this construction as by the former, since (drawing rd perpendicular to Or)

$(rl)^2$ = rect. under segments on either side of r, of chord of which pn is part
$= (rd)^2$;

that is

$$rl = rd = \mathrm{O}e.$$

The Actual Construction for Axis and Equator of Mars.

In fig. 4 the actual construction for two cases (with the full result for one only) is shown; and this part of the present Paper is all that the non-mathematical student of the planet need follow.

First, the circle EPE′P′ is drawn with diameters POP′, EOE′ at right angles to each other. Arcs Pp, Pn are taken on either

side of P, each measuring an angle of $39\frac{2}{3}°$ (I); *pon* is drawn and the half-circle *nlp*; and P′P is produced to *l*.

So much is common to all cases. (This part of the construction may with advantage be carefully drawn in ink on card, to be traced off when wanted; though this will depend on the use to which the constructions are to be applied.) We may add a mark at N, taking PN $= 47\frac{1}{2}° =$ N; but this mark need not actually be used, seeing that it is as convenient to take $(\alpha - \mathrm{N})$ at once.

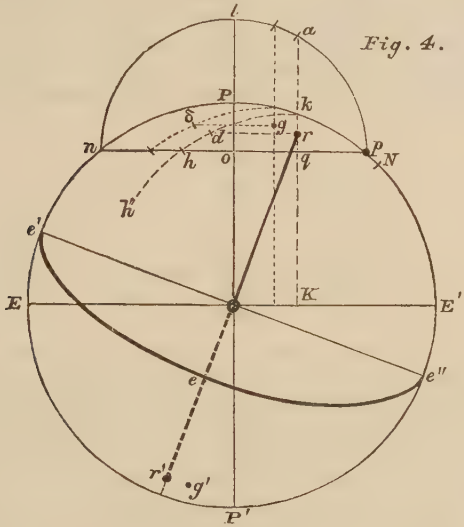


Fig. 4.

Now let the R.A. and Decl. of *Mars* at the epoch dealt with be α and δ respectively.

With a protractor take *pa* measuring angle $(\alpha - \mathrm{N})$ about *o*; and draw *aq*K perpendicular to *pn* and E′E. About K sweep out the arc *khh′*. With a protractor take *hd* measuring arc δ about K. Then *dr* perpendicular to *k*K gives *r* the north pole of *Mars*; *r*O*r′* (O*r′* = O*r*) is the polar axis; *e*O*e′* is the major semi-axis of Martian equator; and O*e* (= *rd*) is the minor semi-axis.

N.B. If $(\alpha - \mathrm{N})$ is between 0° and 180°, *q* lies on the convexity of the sphere PP′. But if $(\alpha - \mathrm{N})$ is between 180° and 360°, we measure this arc from *p* to *n* and then from *n* back to *p* (to avoid drawing the lower semicircle) and *q* lies on the concavity of the sphere PP′. In the former case, northern declinations are measured from *h* towards *k*, and southern declinations from *h* towards *h′*. In the latter case northern declinations are measured from *h* towards *h′*, and southern from *h* towards *k*.

The construction for a second case, $(\alpha - \mathrm{N})$ a few degrees greater and δ also, is shown by the dotted lines. The resulting positions of the poles are shown at *g* and *g′*; and *g*δ is the resulting length of the minor semi-axis of the Martian equator. It is not

necessary, in preparing for an approaching opposition of *Mars*, to make more than five, or at the most seven constructions, the middle one for the day of opposition, since the intermediate positions of the pole and equator of *Mars* can be readily estimated with amply sufficient exactness.

In a future paper I propose to make a few remarks (i) on the completion of the projection of a planet's disk after the position of the poles and equator have been determined; (ii) on the determination of the central areographic longitude for any given epoch; and (iii) on the determination of the centre of illumination.

On the Proper Motion in Right Ascension of η^2 Draconis.

By A. W. Downing, Esq.

My attention was attracted to this star by reading in the preface to the *Nautical Almanac* that the proper motion adopted was that given in Mädler's *Bradley*, and on looking further into the matter I find that from 1848 to 1873 the proper motion in R.A. used in the *Nautical Almanac* was $+0^s\cdot023$, this being the value given in the British Association Catalogue, and since that time $+0^s\cdot005$, taken from Mädler's *Bradley*, has been used. The British Association Catalogue value, $+0^s\cdot023$, has also been adopted in the Greenwich Catalogues since 1845, the Right Ascension of η^2 *Draconis* not being given in the *Fundamenta*, though the Declination is, and consequently the proper motion in the former element has not been computed by Mr. Main or Mr. Stone, whose determinations are based on the *Fundamenta*. The proper motion used in the American *Nautical Almanac* is $+0^s\cdot0024$. As I found that these three authorities differed, I determined to calculate the proper motion in R.A. afresh from the materials afforded by the best available Catalogues.

For this purpose I have, by the kind permission of the Astronomer Royal, used as an assumed R.A. for 1872·0 the place given in the Greenwich Nine-Year Catalogue, so that—supposing the proper motion $=0$—we have for any time

$$\alpha = 16^h\ 22^m\ 15^s\cdot668 + 0^s\cdot8017\ (t-1872) + 0^s\cdot0000939\ (t-1872)^2, \qquad (1)$$

and from this formula I have computed the R.A. for the epoch of each of the Catalogues used.

The following is a list of the Catalogues:-

Catalogue	Mean Epoch	Year of Obs.	Weight
Piazzi	1800	1800	2
Groombridge	1810	1806·8	2
Struve (Cat. Gen.)	1830	1824	3
Pond	1830	1830	2
Madras (Taylor)	1835	1835	½
Armagh	1840	1832·6	½
Greenwich	1840	1839	4
Washington (Gilliss)	1840	1841·5	2
Radcliffe	1845	1841·9	3
Greenwich	1845	1844	4
Poulkova	1845	1849·7	6
Greenwich	1850	1851·1	4
Washington	1860	1852·5	2
Greenwich	1860	1858·1	4
Radcliffe	1860	1858·5	2
Greenwich	1864	1865·1	3

The places given in the various Catalogues have been reduced to Professor Newcomb's Standard (*Washington Observations for* 1870, *Appendix III.*), and have also been corrected for the part of the proper motion, if any, which has been applied to the observed place in reducing it to the epoch of the Catalogue. Then the differences between the Catalogue places thus corrected and the R.A.'s computed by means of formula (1) have been found; whence, by the method of least squares, we have the normal equations:—

$$44\,\Delta\alpha - 1291\,\mu = +\ 2^s{\cdot}5,$$
$$-1291\,\Delta\alpha + 49354\,\mu = -26^s{\cdot}494,$$

where $\Delta\alpha$ is the correction to the assumed R.A. and μ is the proper motion in R.A. for 1872; therefore

$$\Delta\alpha = +0^s{\cdot}177,$$
$$\mu = +0^s{\cdot}00408,$$

and (1) becomes

$$\alpha = 16^h\ 22^m\ 15^s{\cdot}845 + 0^s{\cdot}8058\,(t-1872) + 0^s{\cdot}0000937\,(t-1872)^2$$

I may add that the values of the proper motion in N.P.D. given by the authorities cited above agree fairly well.

On a New Variable in the Constellation Ara.

By J. Tebbutt, Esq.

I have recently been engaged in a re-examination and reduction of the stars compared by me with Comet III., 1862, with a view to the publication of my collected astronomical observations. The observations of that comet were made previously to the erection of my Observatory, and with a ring micrometer on my old telescope of $3\frac{1}{4}$ inches aperture, the instrument being mounted on an ordinary tripod without divided circles of any kind. The method which I adopted for the identification of the smaller comparison stars was to observe their transit, across either the ring or the field of the telescope, in conjunction with some star visible to the naked eye and differing but little from them in declination, the instrument, of course, remaining undisturbed in the interval. The bright star was then identified by sextant observations, and hence the approximate position of the small comparison star became known. It was in this way that I identified the stars *Brisbane* 5754 and 5799, compared with the comet on the evening of October 4, 1862, the bright star being about an hour of Right Ascension further east. The two stars were again compared with the bright one on the evenings of the 5th, 6th, and 9th, and the position of the latter carefully fixed on the last-mentioned date by means of sextant distances from θ *Scorpii*, *Altair*, *Antares*, and ε *Sagittarii*. These observations showed that the star was not to be found in the B. A. Catalogue, or even in the exhaustive one of Lacaille. My journal states that σ *Aræ* and this star were both visible to the naked eye, the latter being about a degree north-east of the former, and the brighter of the two. The journal also states that the star was of the fifth magnitude, and had a small round nebula to the north of it and in the same field of view. A careful reduction of the sextant observations from *Altair* and *Antares*, regard being had to mean refraction only, for temperature 50° and pressure 29·6 in., gives for the apparent place of the star on October 9, 1862, R.A. $= 17^h\ 29^m\ 6^s{\cdot}60$, N.P.D. $= 135°\ 23'\ 42''{\cdot}8$, which, reduced to the mean place for 1862·0, is R.A. $= 17^h\ 29^m\ 2^s{\cdot}51$, N.P.D. $= 135°\ 23'\ 33''{\cdot}8$. Adopting $+4^s{\cdot}415$ and $+2''{\cdot}69$ as the annual precession in R.A. and N.P.D. respectively, I get the following as the mean position for the beginning of 1878:—R.A. $= 17^h\ 30^m\ 13^s{\cdot}15$, N.P.D. $= 135°\ 24'\ 16''{\cdot}8$. There can be no doubt about the approximate position from the sextant observations from *Altair* and *Antares*, as it is verified by comparison with the observed distances from θ *Scorpii* and ε *Sagittarii*. Since the year 1862 my library has been enriched by various Catalogues, but I could not find in any of them a star position corresponding to the above. My curiosity being aroused, I turned the $4\frac{1}{2}$-inch Equatoreal on the spot in the evening of the 13th instant, and was surprised to

find that the field contained stars of the 10th and 11th magnitude only. One star in particular attracted my attention: it was extremely faint, and could only be observed with great difficulty. By clamping the lower circle I measured the difference of Right Ascension between this star and θ *Scorpii* and σ *Aræ*, the difference of North Polar Distance being obtained from the readings of the declination circle. These comparisons gave the following position for the faint star:—R.A. = $17^h\ 30^m\ 21^s$; N.P.D. = $135°\ 23'$. The star was observed on the 13th, 14th, and 17th instant. Although there are several other faint stars within a few minutes of arc of this one, it is extremely probable that the star thus observed is identical with the star of the fifth magnitude by which the positions of *Brisbane* 5754 and 5799 were determined on the 4th, 5th, 6th, and 9th October 1862. The nebula seen in the same field of view with it is undoubtedly No. 3690 of Sir J. Herschel's Cape Catalogue for 1830. As the Variable is a remarkable one, I propose, when the Moon's absence allows the stars to be more readily seen, to record the positions of all the stars embraced in the same field of view.

Windsor, N. S. Wales,
November 21, 1877.

Improvements in a Solar Spectroscope, made by Mr. Grubb for Professor Young. By Wentworth Erck, Esq.

The usual practice in observing with the spectroscope, I believe, is to adjust the collimating and observing telescopes so as to render the incident and emergent pencils parallel for rays of mean refrangibility; and, on observing at either end of the spectrum, then to readjust the eye-piece of the observing telescope so as to render the emergent pencil parallel.

But this mode of procedure will not give the best definition, which requires that each pencil—that is to say, the pencil first incident on the first prism, as well as the pencil after dispersion—should be strictly parallel; and this adjustment of two variables can scarcely, so far as I am aware, be made while the spectroscope is in position.

Now, in the improved spectroscope alluded to, the collimating and observing telescopes are placed close together and parallel to each other, so that the observing telescope shall receive the rays after two internal total reflections.

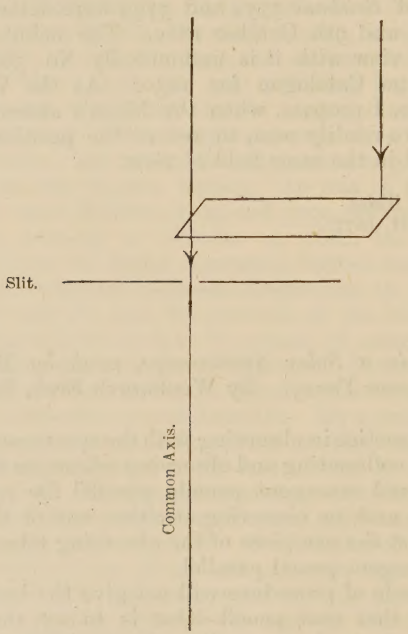
In this position the eye-piece of the observing telescope and the collimating lens are coupled together by rack-work, so that both move together by turning one pinion; and thus, after the first adjustment, the two pencils are rendered strictly parallel for all parts of the spectrum.

The power varies from 1 to 10 prisms of 60°.

The second improvement is in the mode of sweeping round the disk of the Sun.

It is not desirable to turn a heavy, complicated, and delicately adjusted instrument upside down, and into every conceivable position: there is great risk of the adjustments being injured by the flexure.

In the new spectroscope the axis of the collimating telescope is made to coincide accurately with the axis of the great telescope; the edge of the disk is then brought opposite the slit by the rotation of a small parallelopipedon of glass whose length is equal to the radius of the disk.



Thus the parallelopipedon alone rotates, while the spectroscope remains stationary.

Thirdly—The exact coincidence of the axes of the great telescope and the collimating telescope is obtained by mounting the spectroscope on a strong steel bar firmly attached to the great telescope, with a suitable adjustment between the bar and the spectroscope.

On a Combined Position and Setting Circle to be used with Large Newtonian Reflectors. By Wentworth Erck, Esq.

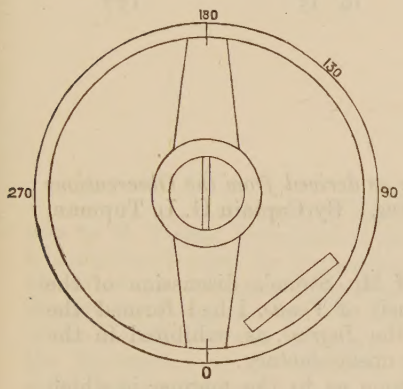
This instrument consists of an ordinary position-circle attached to the west side of the telescope, in such a manner that the diameter 90–270 shall be parallel to the axis.

The rotating eye-piece carries a pair of verniers, parallel to the lines in the eye-piece, in the usual way.

Attached to these verniers is a bubble in such a position that the diameter indicated by the bubble forms with the diameter indicated by the vernier an angle equal to the latitude.

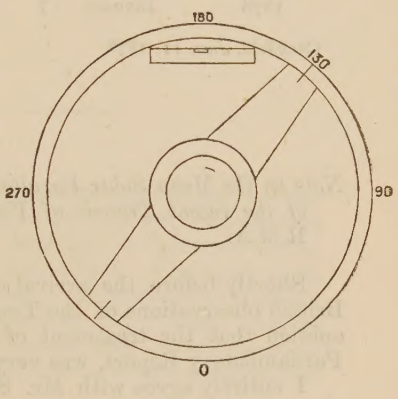
Under these circumstances it is evident that the vernier, when set by the lines in the eye-piece, will give the position of those lines; but when set by the bubble, the same verniers will give on the same circle the N.P.D. of the object to which the telescope is directed, provided the telescope be turned into the meridian.

Fig. 1.



Position Circle, reading 0—180.

Fig. 2.



Setting Circle, reading 129.

Telescope directed to an object in the Horizon.

When reading N.P.D. the circle rotates round a diameter fixed in position by the bubble; whereas, when reading positions a constant diameter rotates round a fixed circle.

It appears to me that in the case of large telescopes the setting circle is very much more accurate, more easily applied, and more economical than the declination circle, and in the case of very large instruments, where the speculum rests on the ground, it is the only mode of getting the N.P.D.

The above arrangement clearly brings the setting circle into the most convenient position for use.

On the Variable Star U. Geminorum. By G. Knott, Esq.

In the month of December last the variable star *U. Geminorum* passed a maximum, which bad weather prevented my observing so completely as I could have wished. The date of maximum was probably about December 17th. The following are all the observations I was able to obtain :—

			h	m		Mag.
1877	December	12	10	15	G.M.T.	13·7
		13	8	45		Not > 13·7
		18	10	40		9·8
		23	12	50		10·9
		24	9	10		11·5
		26	9	5		13·5
		27	8	10		13·6
1878	January	7	10	15		13·7

Cuckfield, Jan. 11, 1878.

Note on the Mean Solar Parallax as derived from the Observations of the recent Transit of Venus. By Captain G. L. Tupman, R.M.A.

Shortly before the arrival of Mr. Stone's discussion of the British observations of the Transit of *Venus*, I had formed the opinion that the treatment of the *Ingress*, as exhibited in the Parliamentary Report, was very unsatisfactory.

I entirely agree with Mr. Stone as to the manner in which these observations should be combined, and I obtain for the *Ingress* a mean solar parallax of 8″·857, with mean error ±·040. As regards *Egress*, Mr. Stone has not yet seen the mass of Australian, Indian, and other observations now lodged at the Royal Observatory. From a discussion of the whole of these, I find the mean solar parallax 8″·792, with mean error ±·027.

From the general solution of the whole, limiting each observer to a single representative time for *Internal contact complete* and rejecting none, I obtain the mean solar parallax 8″·813, with mean error ±·033.

It appears to me that it is impossible to deduce from the observations a parallax much larger than this. Any other legitimate rendering of the Honolulu observations would very slightly reduce it.